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THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

RECENT PROGRESS IN SPECTROSCOPIC METHODS [1]

AN observer who for the first time views the light of the sun through a prism can not fail to express his wonder and delight at the gorgeous display of colors into which the white light is separated—and if the observation is made under the same conditions as in the celebrated experiment of Newton, 1666, there is in truth nothing else which he could observe. You will remember that he allowed a beam of sunlight to stream through a round opening in a shutter of his window, falling on a glass prism, which bent the sun rays through different amounts depending on their color, thus spreading out the white round sunlit spot on the opposite wall into a colored band—the spectrum—which he rather arbitrarily divided into seven colors—red, orange, yellow, green, blue, indigo and violet. (If the division were made to-day I doubt if indigo would be included.) There is in fact no definite demarcation between these, and they shade insensibly into each other—and if the solar spectrum were always produced under these conditions we should say it was continuous, indeed if it were not the sun but an argand burner or an incandescent lamp which served as source, it would really be so.

But even if the source consisted of isolated (but sufficiently numerous) separate colors, the fact would be disguised by the overlapping of the successive images. In other words the spectrum is not pure. In

[1] Address of the president, Washington meeting, December, 1911.

order to prevent this overlapping, two important modifications must be made in Newton's arrangement. First the light must be allowed to pass through a very *narrow aperture,* and second, a sharp *image* of this aperture must be formed by a lens or mirror.

The first improvement was introduced by Wollaston in 1802, who writes:

> If a beam of daylight be admitted into a dark room by a *crevice* 1/20 of an inch broad and received by the eye at a distance of 10 or 12 feet through a prism of flint glass held near the eye, the beam is seen to be separated into the four colors only, red, yellowish green, blue and violet. . . . The *line* that bounds the red side of the spectrum is somewhat confused. . . . The *line* between the red and green . . . is perfectly distinct; so also are the two limits of the violet. There are other distinct lines (in the green and blue . . .).

The second improvement was effected by Fraunhofer, 1814, and by observing the light which fell from such a narrow aperture upon a prism by means of a *telescope* he discovered upward of 750 *dark lines* in the solar spectrum, and mapped their position and general character.

In recognition of the enormous importance of this discovery, these lines are always known as the Fraunhofer lines.

A minor inconvenience in Fraunhofer's arrangement lay in the fact that the slit source had to be at a considerable distance from the telescope; and this was obviated in the apparatus of Bunsen and Kirchhoff, 1860, which is essentially the same as the modern spectroscope of to-day; consisting of a slit and collimator, prism and observing (or photographic) telescope.

And on this beautifully simple device rests practically the whole science of spectroscopy, with all its wonderful applications and all the astonishing revelations of the structure and motions of the sidereal universe, and of the constitution of the atoms of matter of which it consists—nay even of the electrons of which these atoms are built!

Without the telescope it is evident that the science of spectroscopy would be as limited in its field as was the science of astronomy without the telescope. It is interesting indeed to compare the progress of the two sciences as dependent on the successive improvements in the two instruments.

Without the telescope nothing could be discovered concerning the heavenly bodies (with the exception of a few of the more evident features of the sun, the moon and the comets) except the brightness and places of the stars, and the motion of the planets—and even these could at best be very roughly determined (say to within one part in five thousand or something over a half minute of arc). Without the telescope spectroscopy would also have been limited to observations of general differences in character of radiations and absorptions, and a rough determination of the *position* of the spectral lines, with a probable error of this same order of magnitude.

In fact the *resolving power* of the eye is measured by the number of light waves in its diameter of the pupil, about 5,000, and if a double star (or a double spectral line) presents a smaller angle than 1/5,000 it is not "resolved." The resolving power of a telescope with a one inch objective would be about 100,000; so that details of the solar and lunar surfaces and of planets, nebulæ and of double stars and star groups can be distinguished whose angular distance is of the order of 1/100,000. The discs of the planets, the rings of Saturn, the moons of Jupiter, and some star groups and clusters, begin to be distinguishable. Our largest telescopes have a resolving power as high as 2,000,000, corresponding to a limit of separation of one tenth of a second.

But in order to realize the full benefit

of the telescope when used with a prism, the latter must be so large that the light which falls upon it entirely fills the object glass. The efficiency of the prism then depends on its size and on its dispersive power.

In order to form an idea of the separating or resolving power in spectroscopic observations it will be convenient to consider the Fraunhofer line *D* of the solar spectrum, or the brilliant yellow line corresponding to the radiation given out by a salted alcohol flame. This Fraunhofer recognized as a double line, and the length of the light-waves of the components are approximately .0005890 mm. and .0005896 mm. respectively. The difference is then 6/5,893 of the whole, or about 1/1,000, requiring a prism of resolving power of 1,000 to separate them. If the prism were made of flint glass with a base of 25 mm. it would just suffice to show that the line was double.

Now we know of groups of spectral lines whose components are much closer than those of sodium. For instance, the green radiation emitted by incandescent mercury vapor consists of at least six components, some of which are only a hundredth of this distance apart, and requiring therefore a resolving power of 100,000 to separate them. This means a glass prism of 100 inches, the construction of which would present formidable difficulties. These may be partially obviated by using twenty prisms of 5 inches each; but owing to optical imperfections of surfaces and of the glass, as well as the necessary loss of light by the twenty transmissions and forty reflections, such a high resolving power has not yet been realized.

The parallelism of the problems which are attacked in astronomy and in spectroscopy is illustrated in the following table. It is interesting to observe how intimately these are connected and how their solution depends on almost exactly the same kind of improvement in the observing instruments, particularly on their *resolving power;* so that not only are the older problems facilitated and their solution correspondingly accurate, but new problems before thought to be utterly beyond reach are now the subject of daily investigation.

	Astronomical	*Spectroscopic*
1.	Discovery of new stars, nebulæ and comets.	Discovery of new elements.
2.	Star positions.	Wave-length of spectral lines.
3.	Double stars and star clusters.	Double lines, groups and bands.
4.	Shape and size of planets and nebulæ. ? Star discs.	Distribution of light in spectral "lines."
5.	Star motions (normal to line of sight). Resolution of doubles, solar vortices, protuberances, etc.	Star motions (parallel with the line of sight). Resolution of doubles, solar vortices, protuberances, etc.
6.		Changes of character and position of lines with temperature, pressure and magnetic field.
7.	Spectroheliograph (Combination of telescope and spectroscope.)	

We must especially note that the newer problems require an enormous resolving power. In the telescope this has been accomplished partly by the construction of giant refractors and partly by enormous reflectors; and curiously enough the same double path is open to spectroscopy; for we may employ the dispersive power of refracting media or the diffractive power of reflecting media. The increasing cost and difficulty of producing large transparent and homogeneous blocks of glass have tended to limit the size and efficiency of lenses and of prisms, and these have

been more or less successfully replaced, the former by mirrors, and the latter by *diffraction gratings.*

These are made by ruling very fine lines very close together on a glass or a metal surface. The effect on the incident light is to alter its direction by an amount which varies with the wave-length—that is, with the color; and a spectrum is produced which may be observed to best advantage by precisely the same form of spectrometer, with a substitution of a grating for the prism.

The dispersion of a diffraction grating depends upon the closeness of the rulings; but the resolving power is measured by the total number of lines. It is important, therefore, to make this number as large as possible.

The first gratings made by Fraunhofer, 1821, contained but a few thousand lines and had a correspondingly low resolving power—quite sufficient, however, to separate the sodium doublet. A considerable improvement was effected by Nobert, whose gratings were used as test objects for microscopes, but these were still very imperfect as spectroscropic instruments, and it was not till Rutherfurd, of New York (1879), constructed a ruling engine with a fairly accurate screw, that gratings were furnished which compared favorably with the best prisms in existence.

With 30,000 lines (covering over 40 mm.) the theoretical resolving power would be 30,000; practically about 15,000—sufficient to separate doublets whose components were only one fifteenth as far apart as those of the sodium doublet.

An immense improvement was effected by Rowland (1881) whose gratings have been practically the only ones in service for the last thirty years. Some of them have a ruled surface of 150 mm. × 60 mm., with about 100,000 lines and can separate doublets whose distance is only one one hundredth of that of the sodium doublet, in the spectrum of the first order. In the fourth order, it should resolve lines whose distance is only one fourth as great.

Practically, however, it is doubtful if the actual resolving power is more than 100,000; the difference between the theoretical and the actual performance being due to the defect in uniformity in the spacing of the grating furrows.[2]

The splendid results obtained by Rowland enabled him to produce the magnificent atlas and tables of wave-lengths of the solar spectrum which are incomparably superior in accuracy and wealth of detail to any previous work; so that until the last decade this work has been the universally accepted standard. With these powerful aids it was possible not only to map the positions of the spectral lines with marvellous accuracy, but many lines before supposed simple were shown to be doublets or groups; and a systematic record is given of the characteristics of the individual lines, for example, whether they are intense or faint, nebulous or sharp, narrow or broad, symmetrical or unsymmetrical, reversed, etc.—characteristics which we recognize to-day as of the highest importance, as giving indications of the structure and motions of the atoms whose vibrations produce these radiations.

One of the most difficult and delicate problems of modern astronomy is the measurement of the displacement of spectral lines in consequence of the apparent change of wave-length due to "radial velocity" or motion in line of sight. This is

[2] This applies to all the Rowland gratings which have come under my notice, with the exception of one which I had the opportunity of testing at the Physical Laboratory, University of Göttingen. The resolving power of this grating was about 200,000.

known as the Doppler effect and had been well established for sound waves (a locomotive whistle appears of higher pitch when approaching and lower when receding) but it was only confirmed for light by Huggins and by Vogel in 1871, by the observation of displacements of the solar and stellar spectral lines on observing in succession the advancing and the receding limb of the sun.

It may be worth while to indicate the accuracy necessary in such measurements. The velocity of rotation of the sun's equator is approximately two kilometers per second, while the velocity of light is 300,000 kilometers per second. According to Doppler's principle the corresponding change in wave-length should be 1/150,000—a quantity too small to be "resolved" by any prism or grating then in existence. But by a sufficient number of careful micrometer measurements of the position of the middle of a given spectral line, the mean values of two such sets of measurements would show the required shift. It is clear, however, that if such radial velocities are to be determined with any considerable degree of accuracy, nothing short of the highest resolving power of the most powerful gratings should be employed.

Another extremely important application of spectroscopy to solar physics is that which in the hands of Hale and Deslandres has given us such an enormous extension of our knowledge of the tremendous activities of our central luminary.

The spectroheliograph, devised by Hale in 1889, consists of a grating spectroscope provided with two movable slits, the first in its usual position in the focus of the collimator, and the second just inside the focus of the photographic lens. A uniform motion is given to the two slits so that the former passes across the image of the solar disc, while the other exposes continually fresh portions of the photographic plate.

If the spectroscope is so adjusted that light of the wave-length of a particular bright line in a solar prominence (say one of the hydrogen or the calcium lines) passes through the spectroscope then a photograph of the prominences, or sun spots or faculæ, etc., appear on the plate. But the character of this photograph depends on the portion of the bright spectral "line" which is effective, and as the entire range of light in such a line may be only a thirtieth part of the distance between the sodium lines, it would require a resolving power of at least 100,000 to sift out the efficient radiations so that they do not overlap.

As another illustration of the importance of high resolving power in attacking new problems, let us consider the beautiful results of the investigations of Zeemann on radiation in a magnetic field. The effect we know is a separation of an originally simple radiation into three or more, with components polarized at right angles to each other. This is one of the very few cases where it is possible to actually alter the vibrations of an atom (electron) and the fact that the effect is directly calculable, as was first shown by Lorentz, has given us a very important clue to the structure and motions of the atoms themselves.

The experiment is made by placing the source of radiation (any incandescent gas or vapor) between the poles of a powerful electromagnet and examining the light spectroscopically. Now this experiment had been tried long before by Faraday but the spectroscopic appliances at his disposal were entirely inadequate for the purpose.

Even in the original discovery of Zeemann only a broadening of the spectral line was observed, but no actual separation. In fact, the distance between components which had to be observed was of the order of a hundredth of the distance between the sodium lines, and in order to effect a clear separation and still more to

make precise measurements of its amount, requires a higher resolving power than was furnished by the most powerful gratings then in existence.

As a final illustration, let us consider the structure of the spectral "lines" themselves. Rowland's exquisite maps had shown many of these which were then thought simple, to be double, triple or multiple, and there are clear indications that even the simpler lines showed differences in width, in sharpness and in symmetry. But the general problem of the distribution of light within spectral lines had scarcely been touched. Here also the total "width" of the line is of the order of one one-hundredth of the distance between the sodium lines and it is evident that without more powerful appliances further progress in this direction was hopeless.

Enough has been said to show clearly that these modern problems were such as to tax to the utmost the powers of the best spectroscopes and the experimental skill of the most experienced investigators.

Some twenty years ago a method was devised which, though somewhat laborious and indirect, gave promise of furnishing a method of attack for all these problems, far more powerful than that of the diffraction grating.

Essentially, the extremely simple apparatus which is called the interferometer consists of two plane glass plates. These can be made accurately parallel and their distance apart can be varied at will. When light is reflected from the surfaces which face each other, the two reflected beams of light waves "interfere" in such a way as to add to each other, giving bright maxima, or to annul each other's effect, producing dark spaces between.

The alternations of light and darkness which occur when the eye observes in the direction of the normal are very marked so long as the plates are very near together—but as this distance increases, the interferences become less and less distinct until at a distance *which depends on the character of the incident light* they vanish completely. A perfectly definite relation holds between the "visibility curve" and the character of the radiation so that the one can be deduced from the other.

Now the "resolving power" of such an apparatus is measured by the number of light waves in the doubled distance between the surfaces. This is about 100,000 for a distance of one inch; but the distance is in fact *unlimited* and as the instrument itself is practically free from errors of any sort, its resolving power is practically unlimited.

The use of this method of light wave analysis is attended with certain difficulties, and the results obtained are not always free from uncertainties; but in view of the fact that at this time no other methods of this power had been devised, it has amply proved its usefulness. Among the results achieved by it may be mentioned: the resolution of many lines supposed single into doublets, quadruplets, etc.; the measurement of their distances apart; the distribution of light in the components; the measurement of their width and the changes produced in them by temperature, pressure, and presence of a magnetic field.

Among the radiations thus examined one proved to be so nearly homogeneous that over two hundred thousand interference bands could still be observed. Otherwise expressed, the exact number of light waves in a given distance, say ten centimeters, could always be determined; and by a comparison with the standard meter the absolute wave-length of this radiation could be measured and made to serve as a basis for all wave-lengths.

The standard of length itself, the standard meter, is defined as the distance between two lines on a metal bar; and notwithstanding all the care taken in its manufacture and preservation, there is no assurance that it is not undergoing a constant slow change, doubtless very small, but appreciable by the refinements of modern metrological methods, if there were any fundamental unchangeable standard with which it could be compared. The earth's circumference was supposed to be such a standard and the meter was originally defined as the millionth part of an earth-quadrant; but the various measurements of this quadrant varied so much that the idea was abandoned. The attempt to base the standard on the length of a seconds-pendulum was no more successful.

But we have now the means of comparing the standard meter with the length of a light wave (the standard meter contains 1,553,163 waves of the red radiation from cadmium vapor) so that should the present standard be lost or destroyed, or should it vary in length in the course of years, its original value can be recovered so accurately that no microscope could detect the difference. True it is that in the course of millions of years the properties of the atoms which emit these radiations and the medium which propagates them may change—but probably by that time the human race will have lost interest in the problem.

The difficulties in the application of the interferometer method of investigating the problems of spectroscopy, it must be admitted, were so serious that it was highly desirable that other instruments should be devised in which these difficulties were avoided. This need was supplied by the "echelon," an instrument based on the same principle as the diffraction grating, but consisting of a pile of glass plates of exactly equal thickness and forming a kind of stairs, whence its name.

The grating acts by assembling light-waves whose successive wave trains are retarded by some *small* whole number of waves (usually less than six, the distance between the grating spaces being about six light-waves), whereas this retardation in the echelon is many thousand.

But the resolving power depends on the *total* retardation of the extreme rays, and this may be made very large, either by having an enormous number of elements with small retardations—or by a comparatively small number of elements with large retardations. For example, an echelon of thirty plates of glass one inch thick, each producing a retardation of 25,000 waves, would have a resolving power 750,000—about seven times that of the grating; and this high value has actually been realized in practise.

Simultaneously Perot and Fabry showed that by the repeated reflections between two silvered surfaces[3] a very high resolving power is obtained, and a few years later Lummer devised the plate interferometer which embodies practically the same idea.

The resolving power of all of these newer devices is clearly many times as great as that of the grating—but all equally share the objection which holds (but to a far less extent) for the grating, that the different succeeding spectra overlap. It is true that this difficulty may be overcome (though with some loss of simplicity and considerable loss of light) by employing auxiliary prisms, gratings, echelons, etc., and in this form all these modern instruments have contributed results of far reaching importance, and which would have been impossible with the older instruments.

[3] Boulouch, 1893, had observed that Na rings were doubled both by reflection (grazing incidence) and transmission (normal incidence) with a light silver film.

The diffraction grating possesses so many advantages in simplicity and convenience of manipulation that it is even now used in preference to these modern instruments, save for such refinements as require an exceptionally high resolving power. But has the resolving power of the grating been pushed to the limit? We have seen that this depends on the number of rulings; and it is certainly possible to increase this number. But the theoretical value is only reached if the rulings are very accurately spaced; for instance, the resolving power of the Rowland grating is only one third of its theoretical value. This is a direct consequence of inaccuracies in the spacing of the lines. If a grating could be constructed of say 250,000 lines with exact spacing, the resolving power would be equal to that of the most powerful echelon. The problem of the construction of such gratings has occupied my attention for some years; and while it has met with some formidable difficulties, it has had a fair measure of success and gives promise of still better results in the near future.

The essential organ in all ruling engines in actual use is the screw which moves the optical surface to be ruled through equal places of the order of a five hundredth to one thousandth of a millimeter at each stroke; and the principal difficulty in the construction of the machine is to make the screw and its mounting so accurate that the errors are small compared with a thousandth of a millimeter.

This is accomplished by a long and tedious process of grinding and testing which is the more difficult the longer the screw. A screw long enough to rule a 2-inch grating could be prepared in a few weeks. Rowland's screw, which rules 6-inch gratings, required two years or more—and a screw which is to rule a grating 15 inches wide should be expected to take a much longer time, and in fact, some ten years have been thus occupied.[4]

I may be permitted to state a few of the difficulties encountered in this work—some of which would doubtless have been diminished if my predecessors in the field had been more communicative.

First, is the exasperating slowness of the process of grinding and testing the screw. This can not be hurried, either by grinding at greater speed, or by using any but the very finest grade of grinding material. The former would cause unequal expansions of the screw by heating; and the latter would soon wear down the threads till nothing would be left of the original form.

Secondly, in ruling a large grating, which may take eight to ten days, the ruling diamond (which must be selected and mounted with great care) has to trace a furrow several miles long in a surface as hard as steel—and often breaks down when the grating is half finished. The work can not be continued with a new diamond and must be rejected and a new grating begun.

Thirdly, the slightest yielding or lost motion in any of the parts—screw, nut, carriage or grating, or of the mechanism for moving the ruling diamond—is at once evidenced by a corresponding defect in the grating. When after weeks or sometimes months of preparation all seems in readiness to begin ruling, the diamond point gives way and as much time may have to be spent in trying out a new diamond.

When the accumulation of difficulties has seemed insurmountable, a perfect grating is produced, the problem is considered solved, and the event celebrated

[4] A method of ruling gratings accurately, which is independent of any mechanical device, is now in process of trial, in which the spacing is regulated by direct comparison with the light-waves from some homogeneous source such as the red radiations of cadmium.

with much rejoicing, only to find the next trial a failure. In fact, more time has been lost through such premature exhibitions of docility than in all the frank declarations of stubborn opposition!

One comes to regard the machine as having a personality—I had almost said a feminine personality—requiring humoring, coaxing, cajoling—even threatening! But finally one realizes that the personality is that of an alert and skilful player in an intricate but fascinating game—who will take immediate advantage of the mistakes of his opponent, who "springs" the most disconcerting surprises, who never leaves any result to chance—but who nevertheless plays fair—in strict accordance with the rules of the game. These rules he knows and makes no allowance if you do not. When *you* learn them and play accordingly, the game progresses as it should.

As an illustration of the measure of success attained in this work, I would call attention to a recent comparison by Messrs. Gale and Lemon of the performance of a grating of 6½-inch ruled surface with that of the echelon, the Perot and Fabry interferometer and the Lummer plate. The test object is the green radiation from incandescent mercury vapor. The spectrum of this radiation had been supposed a simple line, until the interferometer showed it to be made up of five or more components. The whole group occupies a space about one fifteenth of that which separates the sodium lines.

The grating clearly separates six components while the more recently devised instruments give from six to nine. Two of these components are at a distance apart of only one hundred and fiftieth of the distance between the sodium lines, and these are so widely separated by the grating that it would be possible to distinguish doublets of one half to one third this value; so that the actual resolving power is from 300,000 to 400,000—of the same order, therefore, as that of the echelon.

It may well be asked why it is necessary to go any further. The same question was put some twenty years ago when Rowland first astonished the scientific world with resolving powers of 100,000—and it was his belief that the width of the spectral lines themselves was so great that no further "resolution" was possible. But it has been abundantly shown that this estimate proved in error, and we now know that there are problems whose solution depends on the use of resolving powers of at least a million, and others are in sight which will require ten million for their accurate solution, and it is safe to say that the supply will meet the demand.

To return to our comparison of the telescope and the spectroscope; while the progress of investigation of the stellar universe will be ever furthered by increased size and resolving power of the telescope, this is very seriously hampered by the turbulence of the many miles of atmosphere through which the observations must be made. But there is no corresponding limit to the effective power of spectroscopes and the solution of the corresponding problems of the sub-atomic structures and motions of this ultra-microscopic universe may be confidently awaited in the near future.

The message we receive from the depth of the stellar firmament or from the electric arcs of our laboratories, come they in a millionth of a second or in hundreds of light years, are faithful records of events of profound significance to the race. They come to us in cipher—in a language we are only beginning to understand.

Our present duty is to make it possible to receive and to record such messages. When the time comes for a Kepler and a Newton to translate them we may expect

marvels which will require the utmost powers of our intellect to grasp.

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AMERICAN SOCIETY OF NATURALISTS

HEREDITY AND PERSONALITY[1]

The fathers of the American Society of Naturalists in their wisdom made the president's address an after dinner speech. What can they have meant by that, save to free him from the shackles of that rigorously scientific procedure which marks our day-light program, to enable him to speak in lighter vein, to discourse of things that as a technical scientist he can not touch; in short, to invite him to leave the solid ground of science, and, following the modern vogue, circle about a bit in the atmosphere above?

And so, in accordance with their prudent provision, I shall neither present to you results of my own experimentation, nor indulge in that favorite present-day pastime of geneticists, so facile when one is far from the material itself, of demonstrating that the experiments of some one else prove just the opposite of what he supposed them to prove. There lacks, alas! no opportunity for disputation in that part of genetics where I am at work, but the problem of pure lines and selection has been at this meeting of the society in more competent hands than my own, and it now needs, not more argument or exposition, but further investigations that shall fulfil the demands of both sides—the analytical experimentation of the pure line worker, the analytical computation of the statistical school—till the two come to some unified result.

So, turning aside from all this, I shall put forth some reflections on the relation of our knowledge of genetics to certain human problems. We ourselves are samples of the material whose rules of action we seek in studying genetics, and one can't help thinking of the bearing of the rules we discover on some of the more intimate questions of human life—even though these reflections may lead nowhere and justify no practical conclusions. Considerations of such a sort are forbidden ground to the man of science in his technical rôle; yet the human being, even though he has been through the scientific mill, is attracted by the forbidden, particularly as an after dinner diversion. We spend our time searching for the practical applications of genetics; it may be a rest from the strain to dally a few moments with the unpractical aspects. I judge that it is clear that what I have to say will have no relation to eugenics.

Genetics is that part of science which deals with the question of how living things have come to be what they are, and with what is to become of them later. Now, these are questions that have long troubled the minds of the living things that make up mankind, with relation to themselves. Shall we lay ourselves open to the charge of audacity, of presumption, even of impiety, if then we try to bring the problems of the origin and fate of human individuals into relation with the science of genetics? Following the admonition of America's philosopher, that we shall do what we are afraid to do, let us venture.

It is popularly held that in the last twenty years genetics has begun to be a science. We seem at last to have gotten hold of some of the threads by which the web unravels, and if the unraveling has not yet gone far, we at least see that the process works; that we make progress at it. It is perhaps no longer an adequate statement of our knowledge to say, as a French author did some years ago:

[1] Presidential address before the American Society of Naturalists, December 28, 1911.

> Heredity is a vain word. There are in it no laws to be drawn forth, and consequently no principles that can be stated. There are simply certain curious remarks that may be made, sometimes for, sometimes against, the transmission of virtues and vices by blood. And there are no more cases for than against.

Perhaps we may say that two chief things have been discovered. One is that there is a certain permanency of type in living things, along with a certain dissectibility, as it were, and a capacity for recombination in diverse ways. Certain traits or characters seem to crystallize out, and such crystallized units hold together, and may be moved about, in the processes of generation, according to certain rules, from one individual to another, and combined with other crystals from a diverse source. Or, to change the figure, we find the living world to be a web or net of definite, relatively permanent strands, that interweave, that unite and separate, a given strand passing now into one individual, now into another; each individual presenting a new combination of the strands; a new knot in the web. And we have worked out certain of the rules according to which this interweaving takes place.

The second great discovery is that of some of the intimate material processes of this interweaving. So far as we have gone, we find that the strands which appear in one view as personal characteristics, physical or mental, appear in another as material processes, visible under the microscope; and the rules for the interweaving that we discover by the study of one aspect of the web we find faithfully followed when we study the other aspect. This correspondence seems to that unscientific wondering individual which every man of science conceals, one of the most astounding things in science; it illustrates again the artless ingenuousness of the popular idea that matter is something simple and well known, and that we deprive a phenomenon of its wonder by showing that it takes place in matter. What happens in the personal world finds its parallel, so far as we can see, in the happenings of matter; the wonder of the event is not increased or diminished whether we must call its medium matter or something else equally mysterious and unfathomable; for nothing could be more so.

Our experimental science of genetics is a physiology of the processes by which new generations are produced, comparable to the physiology of metabolism—rather than a study or doctrine of evolution; although we believe, and perhaps we see, that a knowledge of it must precede any correct understanding of evolution. Indeed, the direct attacks hitherto made on the problem of how evolution occurs seem to owe their relative lack of success to the fact that they were not based on a knowledge of the normal physiology of generation; to obtain this preliminary knowledge is now the immediate task of investigation. But this gives us as yet little or nothing that is final on how the strands that make up the living web arise, how they get their unity and permanence, and how they are transformed. Selection, mutation, environmental action, formation of developmental habits—each of these stands before us with a question mark so large as to overshadow the word itself; experimentation finds it equally difficult to confirm any of them.

But the existence and interweaving, according to rules, of these relatively permanent strands, are what remain to us positively. What is the relation of these things to our own existence and personality?

As a material, potentially visible organism, I, like the infusorian, have been in existence ever since the race that devel-

oped into human kind began. And this, for each of us, is not a figure of speech, but the plain literal truth. An unlimited microscopist could have followed with his eyes my course, and your course, down through countless ages, never losing sight of the material organism for an instant, just as our colleague, Dr. Woodruff, follows day by day his thousands of generations of *Paramecium*. I was in actual material existence as a living organism, and indeed thousands or millions of years old, when the pyramids were built, and my unlimited microscopist could give my history from that time to this without a break. What marks has that long history left on my personality and character?

When in England for the first time last summer, I was struck with the familiarity of things strange; by a feeling as if I had returned to my old home. The great things of England seem the working out, the carrying to a limit, as it were, of the tastes that live in me and mine, while the great things of other countries are the revelation of a spirit to me relatively new and foreign. It may not have been an explanation, but it was the truth when I said to myself at that time: I have indeed lived in England many hundred years, much longer than I have lived in America. During the thousands of years of my existence I have had experience of many lands and many people. But of the last thousand years of my life, I have spent all but a couple of centuries or so in England. During that time I have taken part in the growth and development of many an Englishman, and of many an Englishwoman. And who can say that what I have grown into in America has not been partly determined by those habits of growth and development that I acquired in that pleasant English country—so that it is small wonder if things there fit me as if they and I were made together?

True, I was but a cell in the bodies of those many Englishmen, but are we sure that that statement has any real meaning; that the cell—even the germ cell—is in any sense a separate thing from the remainder of the body? Must we not rather conceive the body as a unit, in which all parts share in the developmental processes that occur? In those activities of organisms that are most readily studied, the principle holds that any process gone through repeatedly and under stimulation later takes place more readily and without the original stimulus. There is no reason why we should not expect this principle to hold in development as well as in the other activities of living things. If the body develops as a unit and each cell in the body takes part in that development, we have the basis required for the operation of this principle. After it has developed in a certain way a number of times under the action of certain environmental stimuli, a piece of the body, forming the germ cell, would later develop in the same way without the same stimuli. What we have been accustomed to develop into for the last several thousand years, under the stimulus of our old homes in Europe, possibly we develop into here, so that our old homes fit us as a mold fits the candle that was shaped in it.

The gradual formation of developmental habits seems the only form of the idea of inheritance of acquired characters that is not opposed by any of the experimental facts, that helps us to understand why so many acquired characters are not inherited—since they are not produced by the developmental processes of the organism; that fits all the recent cases which give positive evidence for the inheritance of acquired characters, and that is based

on a law actually known to hold for those organic processes that are most favorable for study with relation to such laws. Can a stronger statement be made for the efficiency of selection or of any other factor, as producing and modifying the characteristics of organisms? There was a time, not distant, when the biologist hardly dared speak of the possibility of the inheritance of acquired characters in any sense, because experimentation was unable to demonstrate its occurrence. But after learning the rules for the interweaving and transfer of characteristics in successive generations, we find as much difficulty in showing experimentally that selection modifies hereditary characters as we do in showing the inheritance of acquired developmental habits, so that the two ideas now stand once more on the same footing. This revolutionary change in the relation of these two possible factors is one of the important fruits of the recent development of genetic science, with its demonstration that most of what had been considered a productive action of selection was in reality not such. If we are reduced once more to judging the two ideas by their relative value for explaining what we find to exist, habit formation in development does not suffer by comparison with selection.

If the formation of developmental habits really occurs, then the fact that each of us has taken part in the development of so many men and so many women, and even, in former times, in the development of so many creatures not yet men and women, helps us to understand many of our impulses, revealed suddenly and unexpectedly to ourselves; helps us realize why we feel that the character and tastes we have manifested in our lives form only one of the types of character that we might have displayed; that perhaps we have displayed in times past.

But however it be with this particular point, I have lived, like the infusorian, in unbroken material continuity for uncounted ages; if the phrase "potential immortality" means anything for the infusorian, it means exactly the same for me, so far as we can judge from past history.

But what then of the future? We have each a singular wish to trace our existence not so much backward as forward; certainly no other problem of genetics has commanded such universal interest as that of immortality.

Many non-scientific theories of immortality have held that we do continue to exist in later generations, in the form of human beings or in other forms, but that we do not remember our previous lives. This last proviso is a relapse into science; it is an attempt to reckon with the facts, for we each observe, upon inspection, that we do not remember a previous existence.

What difference would there be between reincarnation without recollection of our previous experiences—and the actual reliving of our characteristics when a portion of our body develops anew the character and traits that now exist in us? If *you* are a reincarnation of some former individual without the remembrance of his experience—and *I* am a re-development of the characteristics of some former individual from a piece of his body—what pragmatic difference, what difference that experiment or experience could detect, would there be between the two cases?

Thus the fact that we re-live in posterity would seem to constitute all that can be meant by immortality without recollection —if we reproduced as the infusorian does, each for himself, each giving rise to individuals like himself.

But just here we meet that tremendous

complication, which confuses the mind on this point, as it does on so many others. How relatively simple a science would biology be, and how totally different from what it is, if there were no intermingling of individuals for reproduction! The next re-development of *me* is not merely myself—my characteristics, but a combination of my characteristics with those of some one else. And not all of my characters go into the new generation, but only a part of them. And still more perplexing, what I contribute to this new generation often turns out not to be my personal characteristics at all, but those of various and sundry other persons scattered along the line down which I have come, and for which I have served merely as a storehouse, without my knowledge or consent.

And in fact, it turns out that *I* have been merely a sort of focus or knot, in which a lot of strands have been tied together—strands that diverge before and behind me. Cut the knot—the strands separate, scatter and unite with others. Those in my knot have come from a hundred others, and may later unite in a hundred still diverse. Of my characteristics I may say, like Iago of his purse "'twas mine, 'tis his, and has been slave to thousands." Only the scattered parts of me will continue to exist, in diverse persons. And so much is already true; my component parts exist at this moment in many persons now alive, so that if the continued existence of my scattered parts is what we must mean by immortality, then such immortality is the lot of all; it holds as well and in the same sense for him who leaves no children of his own as for the parent. The conclusion of the whole matter, from this point of view, can be only that humanity is but a single organism, merely temporarily separated into pieces, which later reunite, and that we personally must console ourselves (if it is a consolation) with the realization that our characteristics exist elsewhere in humanity and will continue to exist after that particular knot which forms the present self has been untied.

But has not our point of view thus far been after all inadequate for sounding the real depths of our problem? It omits the deepest of all the difficulties; the fact that *I*, the ego, as a feeling, experiencing, knowing self, am identified with only one of these knots into which the living strands are tied; my experiences cling to that one alone. Was it the small boy Huxley (or was it some other one of the famous precocious youngsters that fulfilled their promise) who asked his mother whether she was not overwhelmed by the consciousness of her own identity? And isn't that the most extraordinary of all things, that my experience, embracing in its grasp the universe, is tied down in relations of identity to a single one of the millions of knots tied in this web of strands that have come down from the unbeginning past? For an observer standing to one side, as it were, it is not difficult to comprehend that different combinations of strands should give different characteristics; different personalities in that sense. But that the observer himself—my total possibility of experience, that without which the universe for me would be non-existent—that this should be given only by one particular combination is hard to conceive.

It is the problem of distribution that here seems to call for analysis. Through the operation of what determining causes is my self—my entire possibility of experiencing this wonderful universe—tied to this particular one of the combinations of strands, rather than to some of the mil-

lions of others? And would *I* never have been, would *I* have lost my chance to participate in experience, would the universe never have existed for me, if this combination had not been made?

There seem to be certain facts that bear upon this question. My self, my personal identity, has as a matter of fact arisen in connection with a particular union of two germ cells each bearing a certain combination of the strands that determine characteristics. The essential question is: Could any other combination have produced *my* personal identity?

We find that other combinations are formed in great number, but that none of these do as a matter of fact produce *my* self, not even when they are combinations of germ cells from the same two parents. Suppose that my particular combination of germ cells had never been made, then seemingly those other combinations that *are* made would produce the same results that they now produce, namely, individuals that are *not-I*. And my personal possibility of experience would have been forever non-existent!

On this basis, what are the chances that *I* should ever have existed; that the particular combination which produced *me* should ever have been made? According to competent authorities, one of the two preexisting combinations from which my combination was derived possessed somewhat more than 17,000 germ cells, while the other produced the very considerable number of 339 billions of germ cells. So far as conditioned by the characteristics of these germ cells, any one of the 300 billions might have united with any one of the 17,000; any combination was *a priori* as probable as any other, and the chance that my particular combination should have been formed was therefore but one in five millions of billions![2] Gentlemen, I must congratulate myself on my fortune in being with you this evening!

But this gives but a minute fraction of the real odds against my existence, or your existence, if each of us depends on the occurrence of some particular combination of the strands. We have taken my two parents and their union as given. But the chances were equally many thousands of billions to one against the existence of each of them, and even existing, they might have mated otherwise, absolutely precluding the possibility of that combination to which my identity and experience are attached; and if we go back many generations, applying as we must the same considerations, we see that the system of notation which humanity has devised would be quite inadequate to express the odds against the formation of the combination from which I was derived, or you were derived. The chances were infinite against my existence and your existence.

As an abstract mathematical proposition, you may, if you like, decline to be impressed with this, because the chances were just as strong against the existence of any other persons, and yet some were bound to exist; you and I were therefore just as probable as any one else. While this reasoning is abstractly just, it fails to be entirely satisfying to the self when it is my total possibility of existence that is disposed of in this light way. But this and all our reasoning thus far omits the essen-

[2] If we choose to take into the computation out of the 17,000 ovules only the 400 that actually mature, the chance for any particular combination is one in 120 thousand billions. After reaching the thousand billions, cancellation of a factor of a few hundreds or thousands ceases to produce an impressive difference. The figures here given for the numbers of germ cells are from the "American Text-book of Physiology," 1901, Vol. II., pp. 444 and 454.

tial point, the real tragedy of the situation. If each diverse combination produces a different *self,* then there existed in the two parents the potentialities—nay, the actual beginnings—of thousands of billions of *selves,* of personalities, each as distinct as you and I. Each of these existed in a form as real as your existence and my existence before our component germ cells had united. And of these thousands of billions, but four or five have come to fruition. What has become of the others? A thousand earths might have been populated with those personalities now consigned to limbo. Or, if, as before, we include in our thought other persons, and previous generations, what must we conclude? A real infinity of potential, of inchoate, selves, is cancelled in each generation; a potential and inchoate population sufficient to people all the regions that mythology has invented; all the worlds that astronomy has discovered.

Our instincts and our education impel us to regard a human personality as the highest and most real of entities, having attributes of worth possessed by nothing else; perhaps as being sacred and imperishable. What are we to say of this infinite number of personalities whose existence was foreshadowed and prepared in exactly the way that gave origin to you and to me; who depended only on a chance meeting of germ cells for their full fruition, yet that never advanced farther?

It has become popular, with the advance of the theory of natural selection, to shudder at the tragical ruthlessness of nature, because, according to the very moderate estimate of the poet,

> of fifty seeds
> she often brings but one to bear.

Many a plant produces thousands of spores for each one that matures, and many a fish produces thousands of eggs condemned to premature destruction. Natural selection has therefore been reproached as a tragic and cruel method of advance, since out of the thousands of inchoate existences it brings but one to fruition. An honored former president of this society has tried to show us that nature acts in a kindlier way, through an attempted demonstration that natural selection is not the correct theory as to the method of advance of living things.[3] But the destruction of the uncounted millions was not a part of the theory; it is an observed fact, for which the theory merely tried to give some sort of an excuse. If no purpose is served, no advance made, through this wholesale slaughter, then mere wanton cruelty is substituted for that cruelty whose aim is kindness. But whether with an aim or without, we find that nature plays in the same infinitely wasteful and cruel way, whether with spores of fungi and eggs of fish, or with the potencies and beginnings of human personalities; it is but one out of billions prepared that comes to fruition.

It is not strange that with the instincts and education which we have, men should turn away from such a view of nature, and should attempt to find some alternative that does not lead to such monstrous results. If we have, from studies in philosophy or in other fields, reached the conclusion that the self is the one certain reality, that relation to its existence is the final touchstone for all knowledge; that it is the highest and greatest thing; that it is as it were self-existent, perhaps even imperishable—then this conviction will appear to us a sound argument against the correctness of a view of nature which shows us the existing human selves as a mere chance

[3] Morgan, T. H., *The Popular Science Monthly,* May, 1905, p. 63.

remnant saved from an infinite slaughter. There exists, as we know, an alternative point of view in regard to human selves; one not reached by following the road that leads from the facts of biological science; one that gives the human self a very different position and relation to the rest of the universe. Is that indeed a real "view" or is it a mere refusal to look at the view which is before us? Is that viewpoint one that could be reached in any way from the biological field? Is there any possibility of reconciling it with the data with which we have been dealing? Can we possibly give our own argument a different direction?

With some ingenuity one might find a parting of the ways at that point in our argument where it was set forth that if *I* did not exist, all the other combinations of germ cells that are made would still produce the same result that they do produce —namely, individuals that are not-*I*, so that I would never have existed. It could perhaps be maintained that, on the contrary, *my* existence is in some way one of the determining factors for what shall be produced by other combinations, so that if I did not already exist, some of those combinations might produce a different result from what they do produce; that they might indeed in that case produce *my* self. Granting this, I might have had my personal existence as a self, in connection with some different combination of the living strands, in case the one I am tied to had not been formed.

To work this out in detail, one would apparently have to hold that the human self is an entity existing independently of the living material, and that it merely enters at times into relations with one of the knots of the living web. If one particular combination or knot should not be produced, it would enter into another. Thus each of us might have existed with quite different characteristics from those which we have; it would be only our specific characteristics that were determined by the chance combinations that happened to be made, not our total existence as a self.

We have recently witnessed the phenomenon of a vice-presidential address before a section of the British Association for the Advancement of Science, which set forth that the facts of physiology suggest the existence of an entity or soul that is essentially independent of the body, merely acting through it.[4] Could not those aspects of genetics to which we have called attention be readily converted, likewise, into an argument, convincing for those already convinced, for the independent existence of the self or soul? The monstrous results to which the straight-forward consideration of the data leads us could be held to demonstrate in themselves that we had gone astray; that at the parting of the ways we must follow the other road, leading to views in harmony with our convictions drawn from other fields. Neglecting all difficult details as to when and how and why the temporary union of self and the body is made—how simple and satisfactory to hold (if you can) that there is a limited store of selves ready to play their part; that the mere existence of two germ cells which may (or may not) unite has no determining value for the existence of these selves, but merely furnishes a substratum to which for mysterious reasons they may become temporarily attached; and that therefore there is no cancellation of billions of inchoate human personalities, such as the other view leads to; that nature does not deal with human selves as with spores

[4] Macdonald, J. S., *Nature*, September 14, 1911, pp. 364–365.

of fungi, or as with an infinitely great sum of figures employed in computations amounting to trillions and quadrillions, all to be canceled save a result expressible in units. And what interesting corollaries might be drawn from such a doctrine, as to the farther independent existence of the selves after the combinations to which they are attached have been dispersed!

Certainly I do not wish to be understood as advocating this second point of view. The experiences of scientific investigation do not convert one to that thoroughgoing pragmatism which holds that satisfaction to our instincts is ground for holding a proposition to be verifiable. But I take it that the function of a scientific exposition is to follow wherever the argument leads, and when the road forks, with no signboard to tell us positively which fork to follow, it must chronicle that fact, and investigate so far as it can the regions into which each fork leads, leaving the question of choice to each person as a person. When the man of science leaves the solid ground and takes to his aeroplane, such a rule is doubtless difficult, for all roads become dim, but it still remains the ideal.

Gentlemen of the society, whether you have followed me in any other respect or not, you will admit the truth of my introductory promise that I would give you a rest from things practical and that I would not try to lead you to any conclusion. Looking at some of the elementary facts of genetics in relation to ourselves, we saw that each of us has been in unbroken material existence for countless ages, during which time we have taken part in the upbuilding of many a brute and many a man and many a woman. After speculating a bit as to the marks which these experiences may have left on our characters, we turned our eyes to the future. We found that each of us is but a knot in a continuous web of strands that have, in other combinations, built up many persons, and will, in still new combinations, build up many others. Thus, as we have before taken part in the development of brute and of man, we may hope later to take part in the development of superman. Finally we looked at the relation of some data of genetics to the problems of personal identity and the self. Here the straight path of science, when followed simply and unsuspectingly, showed us nature cutting off budding human personalities by the billion, where she brings one to fruition. Whether this ingenuous and unforeseeing pursuit of the scientific path as marked out by the objective data is the only proper method for the establishment of belief on such a point or whether we are justified in turning off at a certain juncture, because this takes us where, for other reasons, we would prefer to go, is a question which leads into broader fields than the experimental science of genetics.

H. S. Jennings

SCIENTIFIC NOTES AND NEWS

The American Association for the Advancement of Science and the national scientific societies affiliated with it are opening at Washington the tenth convocation week meeting as this issue of Science is sent to press. There are published above the presidential addresses of Professor Michelson before the American Association and of Professor Jennings before the American Society of Naturalists. These will be followed by other addresses and by the proceedings of the meetings.

Dr. K. von Goebel, professor of botany at Munich, Dr. Aurel Voss, professor of mathematics at Munich, and Dr. Ewald Hering, professor of physiology at Leipzig, have been elected knights of the Bavarian Maximilian order for art and science.

Professor W. C. Brögger (Christiania), Professor T. Curtius (Berlin), Professor P. A. Guye (Geneva) and Professor H. Rubens (Berlin) have been elected honorary members of the Royal Institution.

Professor Waldeyer, of Berlin, has been elected president of the International Committee of the forthcoming International Congress of Medicine, in the room of the late Dr. Pavy.

Professor John F. Hayford, director of the College of Engineering, Northwestern University, has been appointed by the chief justice of the United States a member of a commission of engineers to obtain the data necessary to settle the boundary between Costa Rica and Panama.

The Academy of Sports of France has awarded its gold medal to Admiral Peary for the "admirable lesson of physical energy and moral courage that you have given to the entire world in the midst of fatigues, sufferings and difficulties, the conquest of the North Pole." The resolution was moved by Dr. Charcot.

A $1,000 industrial fellowship has been given the College of Agriculture of the University of Wisconsin for the purpose of studying pea diseases with a view to their prevention. R. E. Vaughn, a graduate of the University of Vermont in the class of 1907, has been appointed to the fellowship for the present academic year.

Mr. D. T. Griswold, of College Station, Texas, has accepted a position to do extension work in agriculture for the Agricultural College of Porto Rico. He will sail for Porto Rico soon.

Professor Newstead has returned from the expedition to Central Africa, on which he had been sent by the Liverpool School of Tropical Medicine in connection with the commission on sleeping sickness, which the government is sending out under Colonel Sir David Bruce.

The chief speaker at the public exercises of Johns Hopkins Commemoration Day, on February 22, will be Dr. S. Weir Mitchell, of Philadelphia. His subject will be "George Washington."

Professor J. McKeen Cattell, of Columbia University, addressed the Huxley Society of the Johns Hopkins University on December 20, his subject being "Some Problems of University Administration."

Professor Charles W. Brown, of Brown University, lectured on November 25 before the Yale Geological Club on "The Human Aspects of the Jamaica (1907) Earthquake," showing how geological investigations should decide upon the location and mode of construction of buildings, and how by the cooperation of geologists and structural engineers the damage from earthquakes in most seismic regions could be almost eliminated. Professor M. L. Fernald, of the Gray Herbarium, Harvard University, lectured before the club on December 7 on "The Distribution of the Coastal Plain and Maritime Plants in North America." The great dominance of the plants of the New Jersey coastal plain in the Newfoundland flora was pointed out and its important geologic significance was emphasized. The inland distribution of maritime plants into the Mississippi valley and over the western part of the continent was also discussed. On December 8, Professor Fernald gave a lecture to the Yale Chapter of the Society of Sigma Xi on "Botanical Evidences bearing on the Exploration of the Norsemen," in which it was shown that the accounts which have been assumed by historians to show their exploration along the eastern coast of the United States did not reach in fact south of the St. Lawrence estuary.

At a representative meeting of former students and friends of the late Professor P. G. Tait, at the University of Edinburgh, it was decided to undertake to establish an additional memorial to him in the form of an endowment of a Tait chair of mathematical physics at the University of Edinburgh.

It is proposed to erect a statue of Joseph Priestley, at Birstall, near which he was born in 1733.

Miss Susan Maria Hallowell, professor emeritus of botany in Wellesley College, where

she had taught since the establishment of the institution in 1875, until her retirement in 1902, has died at the age of seventy-six years.

Mr. Arthur Cottam, who, while engaged in the service of the British government, carried forward valuable work as an amateur astronomer, died on November 23, aged seventy-five years.

Dr. Giorgio Spezia, professor of mineralogy at Turin, died on November 10 at the age of sixty-nine years.

Nature states that the premises of the Institute of Chemistry, the lease of which will expire shortly, and can not be renewed, have become inadequate for the increasing activities of the institute. To carry on the work, the council of the institute requires new buildings, which should include more commodious meeting rooms, library, laboratories, examination rooms, and offices. It is proposed to begin the preparation of plans next year, and it is estimated that the necessary building and fittings will cost about 15,000 *l.* An appeal has been made to fellows and associates of the institute, which has already resulted in the receipt of contributions and promises amounting to more than 8,000 *l.*

A bill (H. R. 14,120) has been introduced in the House of Representatives by Congressman J. Hampton Moore, which calls for an appropriation of $80,000 to enable the Secretary of Agriculture, in cooperation with the various state authorities, to take necessary measures for checking the chestnut tree blight. Of this amount, $20,000 is to be immediately available, and $10,000 is to be spent in studying the relations of insects to the disease. A bill carrying essentially the same provisions (S. 3,557) has been introduced in the Senate by Senator Penrose.

Representatives of the Imperial Health Office, of the medical faculties and a number of journalists met on December 20 at the Ministry of the Interior at Berlin and organized a committee with the object of promoting German participation in the fifteenth International Congress on Hygiene and Demography to be held at Washington in September, 1912.

The Medical Record states that the American Association for the Conservation of Vision is inaugurating a wide-spread campaign of public education to call the attention of people to the care and preservation of their eyesight. The association has recently moved to new offices at 105 East Twenty-second street, New York City. A recent election of officers leaves the personnel as follows: *President,* Dr. F. Park Lewis; *Vice-President,* E. L. Elliott; *Acting Secretary,* Douglas C. McMurtrie; *Acting Treasurer,* T. Commerford Martin. Dr. Hiram Woods, of Baltimore, is on the board of managers and Dr. G. E. de Schweinitz, of Philadelphia, is director of the Department of Diseases and Defects of the Eye. Among the publications of the association are its *Bulletin* and *Monograph Series,* the first of a popular and the latter of a technical nature. The first issue of the *Bulletin* is entitled "Conserving Vision," compiled by Douglas C. McMurtrie and edited by G. E. de Schweinitz, M.D., F. Park Lewis, M.D., Louis Bell, Ph.D., and E. Leavenworth Elliott. The first issue of the *Monograph Series,* edited by Douglas C. McMurtrie, is entitled "Ophthalmia Neonatorum in Ten Massachusetts Cities" by Henry Copley Greene. The association has now in press additional booklets of a popular nature.

The state school fund of Wisconsin will soon be distributed to the various school districts of the state. The per capita apportionment for persons of school age is $2.783, as compared with $2.423 last year. It is a surprising fact that there are 6,236 fewer persons of school age reported for the year ending June 30, 1911, than for the year ending June 30, 1910. The loss in the number of persons of school age is pretty well distributed over the state. Excluding cities under city superintendents, only 24 of the 71 counties show a gain. The increase ranges from 614 for Clark County to 3 for Langdale County. Of the 68 cities under city superintendents 38 show a gain in school population, the largest gain, 767, being in Milwaukee.

A correspondent calls attention to the fact that *Nature* says: "It is proposed to establish

a post of demonstrator in medical etymology in connection with the Quick Laboratory. The appointment will be made by the Quick professor of biology." He suggests that the Slow professor of philology may some day appoint a demonstrator of oriental entomology.

Carnegie University, at Wilmington, Del., states in its announcement that it is "the oldest and most celebrated institution of learning of its kind in the United States of America" and that "by virtue of the powers invested in the university by the government of the state of Delaware" it confers numerous degrees, including M.A., Ph.D., Sc.D., M.D., LL.D., etc. A member of the staff of the *Journal* of the American Medical Association wrote that he was unable to take the regular course, but would pass the examination if the university would send him the examination papers. Among the questions and the answers submitted were the following:

Question—What is histology?

Answer—Histology is the study of the history of the anatomy and physiology of the body.

Question—What is embryology?

Answer—Embryology is the study of the newborn baby and how to care for it.

Question—Describe the portal circulation.

Answer—The portal circulation is the circulation of the chyle and chyme which is found in the stomach when the food is being digested. It then goes into the blood to build up the body.

Question—Describe the fornix.

Answer—The fornix is that part of the throat at the back of the tonsils which is affected in catarrh. An adjustment of the vertebra of the neck will often help it.

Question—How would you replace a dislocated lower jaw?

Answer—The jaw should be pulled forward or pushed back, as the case may be, and the joint massaged and adjusted.

Question—Give pathology, etiology, symptoms and treatment for malaria.

Answer—Malaria is found in the south and in swampy places. The patient should be given massage to make the bowels move and the spine should be adjusted to improve the circulation. It is also better to have the patient move from a malarial place to where it is dry.

The action of the "University" was given in a letter which begins:

We herewith have the pleasure to inform you that you have passed your examination very satisfactorily, and that the Carnegie University has conferred on you the degree of Doctor of Mechano-Therapy. The diploma will be forwarded to you on receipt of post-office money order of $50.

A course of sixteen lectures on economic agriculture is offered at Columbia University, beginning with an introductory lecture on Wednesday, November 22, 1911, at 4:30 P.M., and continuing on successive Wednesdays (except from December 20 to January 3 inclusive). These lectures, while dealing with the scientific aspects of the subjects announced in the course, will be divested as much as possible of technicalities. The program is as follows:

November 22—"How a City Man can Succeed in Farming," Professor O. S. Morgan, Columbia University.

November 29—"Agricultural Possibilities about New York City," Mr. George T. Powell.

December 6—"Soil Bacteria—their Importance and How to Control them Advantageously," Director Jacob T. Lipman, New Jersey Agricultural Experiment Station.

December 13—"Practical Problems in Developing the Dairy Herd," Professor Henry Wing, Cornell University.

January 10—"Corn Growing in the East," Director Thomas F. Hunt, Pennsylvania State College of Agriculture.

January 17—"The Farmer as a Plant Breeder," Hon. W. N. Hays, assistant secretary of agriculture.

January 24—"Problems in Feeding the Dairy Herd," Professor E. S. Savage, Cornell University.

January 31—"Poultry Raising," Dr. Raymond Pearl, State Agricultural Experiment Station, Orono, Maine.

February 7—"Soil Drainage Problems and Practises in New York State," Professor E. O. Fippin, Cornell University.

February 14—"Fundamental Problems in Maintaining Soil Fertility," Dr. O. Schreiner, Bureau of Soils.

February 21—"Truck Farming and its Prob-

lems near Great Cities,'' Professor R. L. Watts, Pennsylvania State College of Agriculture.

February 28—''Peach Orchards,'' Professor M. A. Blake, New Jersey Agricultural Experiment Station.

March 6—''Planting an Orchard,'' Dr. U. P. Hendrick, Agricultural Experiment Station, Geneva, N. Y.

March 13—''Orchard Management—with special reference to Fertilization and Spraying,'' Dr. J. P. Stewart, Pennsylvania State College.

March 20—''Problems in Eastern Farming.'' Lecturer announced later.

March 27—''Practical Considerations in Farm Management,'' Dr. W. J. Spillman, Bureau of Plant Industry.

The following lectures on zoological subjects will be given at Trinity College during the course of this year.

December 15—Raymond C. Osburn, acting director of the New York Aquarium and associate professor of zoology, Columbia: Fishes.

January—Frederic S. Lee, director of the department of physiology, College of Physicians and Surgeons, New York: Some Aspects of Muscle Action.

February—George H. Parker, professor of zoology, Harvard: Some Phases of the Nervous System.

March—Professor Henry A. Perkins, Trinity: The Brownian Movement of Ultramicroscopic Particles.

May—Dr. David Dwight Whitney, of Wesleyan: Some Problems in Sex.

April—Irving A. Field, United States Bureau of Fisheries: Utilization of hitherto unused Fishes as Food.

The faculty of Medicine of Harvard University offers a course of free public lectures, to be given at the Medical School, Longwood Avenue, Boston, on Sunday afternoons, beginning January 7, and ending May 5, 1912. The lectures will begin at four o'clock and the doors will be closed at five minutes past the hour.

January 7—Dr. F. C. Shattuck: Catching Cold, etc.

January 14—Dr. John Lovett Morse: Feeding of Infants.

January 21—Dr. Myles Standish: The Care of the Eyes.

January 28—Dr. S. B. Wolbach: A Medical Expedition to West Africa.

February 4—Dr. Abner Post: Syphilitic Heredity.

February 11—Dr. E. E. Southard: The Mental Life in the Light of Modern Efforts to Map the Brain.

February 18—Dr. Charles S. Minot: The Human Face.

February 25—Dr. Joel E. Goldthwait: The Effect of Posture upon the General Efficiency of the Human Being.

March 3—Dr. C. P. Putnam: The Care and Training of Children.

March 10—Dr. Maurice H. Richardson: Conservation, not Destruction, the Chief Object of Surgical Endeavor.

March 17—Dr. Charles J. White: Possibilities of Infection of the Skin in Public Places.

March 24—Dr. E. H. Bradford: Some Causes of Backache.

March 31—Dr. George Burgess Magrath: The Massachusetts System of Medico-legal Inquiry.

April 7—Dr. Charles M. Green: Certain Topics in the Hygiene of Women. (To women only.)

April 14—Dr. E. H. Nichols: The Sexual Instinct—Its Abuse and Control. (To men only.)

April 21—Dr. John Bapst Blake: Fractures, Sprains and Minor Injuries: Diagnosis and Treatment. (Illustrated by lantern slides.)

April 28—Dr. George T. Tuttle: Some Forms of Mental Disease and the Methods now employed in their Treatment.

May 5—Dr. C. J. Blake: The Prevention of Unnecessary Noise.

The home universities committee of the Congress of the Universities of the British Empire, consisting of the vice-chancellors of the universities of the United Kingdom and other representatives, have prepared the program of subjects for discussion at the congress in July, 1912. The meetings of the congress will be held on July 2, 3, 4 and 5, on four mornings and two afternoons. There will be, in addition, a business meeting. The subjects for discussion fall under two heads, and are as follows:

I. Universities in their relation to one another:

1. Conditions of entrance to universities and the possibility of equivalence and mutual recognition of entrance tests to degree courses.

2. Interchange of university teachers; conditions of interchange.

3. Interuniversity arrangements for post-graduate and research students.

4. Question of division of work and specialization among universities.

5. The establishment of a central university bureau; its constitution and functions.

II. Universities in their constitutional aspects and in their relation to teachers, graduates and students:

1. The relation of universities to technical and professional education and to education for the public services.

2. Provision of courses of study and examinations for other than degree students, including university extension and tutorial class work, and specialized courses both of a general and technical character for students engaged in professional, commercial and industrial pursuits.

3. The representation of teachers and graduates on the governing body of a university.

4. Action of universities in relation to the after-careers of their students.

5. The position of women in universities.

6. The problem of universities in the East in regard to their influence on character and moral ideals.

7. Residential facilities, including colleges and hostels.

THE "Quarterly Return of Marriages, Births and Deaths," published by the authority of the registrar-general and abstracted in the London *Times*, shows a remarkable decline in the "natural increase" in population in England and Wales by excess of births over deaths. During the three months there were only 81,645 more births than deaths as compared with 123,300, 124,054 and 123,022 in the third quarter of 1908, 1909 and 1910, respectively. The births registered in the third quarter of 1911 numbered 222,601 and were in the proportion of 24.4 annually per 1,000 of the population, which is 2.9 per 1,000 below the mean birth-rate in the ten preceding third quarters, and it is the lowest birth-rate recorded in any third quarter since the establishment of civil registration. In registration counties with populations exceeding 100,000, the lowest birth-rates during the quarter were 18.4 in Sussex, 20.0 in Northamptonshire, 20.1 in Berkshire, 20.2 in Devon, Somerset and Carnarvonshire. The highest rates were 26.7 in Northumberland, 26.8 in Carmarthenshire, 27.3 in Nottinghamshire, 27.7 in Staffordshire, 29.9 in Monmouthshire, 30.9 in Durham and 31.7 in Glamorganshire. In the 77 great towns the birth-rate averaged 25.5 per 1,000, ranging from 15.8 in Bournemouth, 16.0 in Hastings, 17.9 in Hornsey, 18.3 in Halifax, 18.5 in Huddersfield and 18.6 in Bradford, to 30.1 in Bootle, 31.3 in Stoke-on-Trent, 32.7 in Merthyr Tydfil, 34.7 in St. Helens and 35.3 in Rhondda. In the 136 smaller towns the mean birth-rate was 23.6 per 1,000, and in the remainder of England and Wales, excluding the 213 chief towns, it was also 23.6. The deaths registered in England and Wales last quarter numbered 140,956, and were in the proportion of 15.5 annually per 1,000 persons living; this rate is 1.7 per 1,000 above the mean rate in the ten preceding third quarters. In registration counties with populations exceeding 100,000, the death-rates ranged from 11.4 in Wiltshire, 11.7 in Somerset, 11.8 in Berkshire and Hertfordshire, 11.9 in Shropshire and 12.1 in Buckinghamshire, to 16.7 in the West Riding of Yorkshire, 17.5 in Glamorganshire, 17.6 in the East Riding of Yorkshire, 18.0 in Staffordshire, 18.5 in Lancashire and 18.6 in Durham. The population of the United Kingdom in the middle of 1911 is estimated at 45,311,078 persons; that of England and Wales at 36,168,750, that of Scotland at 4,766,860 and that of Ireland at 4,375,468. These estimates are based upon the numbers enumerated at the censuses of 1901 and 1911. In the United Kingdom 277,655 births and 173,105 deaths were registered in the three months ended September 30, 1911. The natural increase of population was, therefore, 104,550. The official vital statistics of France for the first six months of 1911 give a total of 385,999 birth and 404,278 deaths, being an excess of deaths of 18,279.

UNIVERSITY AND EDUCATIONAL NEWS

AT a meeting of the lumbermen of the North Idaho Forestry Association held in

Spokane on December 16 the members voted unanimously to pro-rate their timber holdings in the state of Idaho to the extent of $58,000 for the purpose of erecting a forestry building at the University of Idaho.

At its meeting on December 15 the board of regents of the University of Michigan took an important step with reference to graduate studies. Hitherto this work has been in charge of a subcommittee of the literary faculty. The recent action (1) founds a university graduate department; (2) provides for the appointment of a dean as chief executive; (3) places the direction of all matters affecting graduate studies in the hands of an executive board of seven, together with the president and dean *ex officiis*. A mixed committee, drawn partly from the administration and partly from the senate, submitted the plan as adopted, after prolonged consideration. This committee was as follows: the President; Regents Sawyer, Beal and Hubbard; Dean V. C. Vaughan, of the medical faculty; Dean John O. Reed, of the literary faculty; Professor R. M. Wenley, of the department of philosophy; Professor F. N. Scott, chairman of the present graduate council, and Professor Alexander Ziwet, of the engineering faculty.

Buildings costing nearly $1,000,000 are either being constructed or will be started at the University of Wisconsin before the next academic year opens. Nine new structures will be completed within the next twelve months on various parts of the university grounds. The new buildings and their cost will be as follows:

Biology hall	$200,000
Wing to library	165,000
Home economics building	115,000
Model high school	150,000
Women's dormitory	150,000
Agricultural chemistry	90,000
Chemistry building wing	76,000
Horticultural building	57,000
Gymnasium annex	15,000
Total	$998,000

The horticultural building is now almost completed and will be ready for classes at the opening of the second semester in February. The annex to the gymnasium will be completed about February 1. The big new biology hall, which will give the department of biology one of the finest homes at any American university, will not be completed before the end of the present school year. Last week ground was broken for three of the new buildings—the women's dormitory, the agricultural chemistry building and the new home economics building, and work will be rushed on all three of these buildings so that they may be ready for occupancy at the opening of the academic year next fall. The basements of both the wing to the chemistry building and that to the library are completed, but further work will not be resumed on them until spring. It is also understood that work on the new model high school, to be built for the use of the students at the university preparing to be teachers, will not be started until spring. This building is to be constructed on University Avenue, nearly opposite the United States Forest Products Laboratory.

The *Journal* of the American Medical Association states that a decree has been promulgated for the improvement of the scientific and clinical education in the French medical schools. Its main features are as follows: (1) the duration of the medical course is increased from four to five years; (2) practical work in physiology and medical physics and chemistry and bacteriology is to be compulsory; (3) the hospital *stage* will be coextensive with the medical course and will include the various services; (4) to decrease the effect of chance in examination, each student will have a record book in which will be noted the credits he has obtained in laboratory and chemical work and previous examinations.

Edward D. Sisson, recently head of the department of education at the University of Washington, has been appointed professor of education in the newly established Reed College, at Portland, Ore.

Mr. Fletcher McFarland has been appointed instructor in physiology at the University of Minnesota.

DISCUSSION AND CORRESPONDENCE

THE POSITIVE ION IN ELECTRICAL DISCHARGE THROUGH GASES

WHEN a metal sphere is hung upon a silk cord between the terminals of a plate glass electric machine, it will oscillate to and fro between the terminals.

When molecules of a gas are placed in a similar position, they can not behave in quite the same fashion. No one molecule can plow its way through the swarm of molecules which surround it. They are all being urged to do this. At any instant some are being urged away from the positive terminal, and some from the negative. These opposing streams of gas mingle. The collisions which result between these overcharged and undercharged molecules within such a field of force result in a continual transfer of electrical corpuscles from molecule to molecule. In such a mixture we should at any instant expect to find three classes of molecules. Those which are negatively overcharged, those which are negatively undercharged, and those which are in normal condition.

Even in open air discharge, the repelled molecules move along streamers. In particular is this the case at and near the positive terminal. Here the corpuscles and air molecules are moving in opposite directions. In rarefied gas, where the mean free path is greatly increased, these streamers become "rays."

All of the properties of these rays are in harmony with what we should expect, from our knowledge of the behavior of the metal ball and the properties of gases.

FRANCIS E. NIPHER

WASHINGTON UNIVERSITY

A NEW RECORD OF A CHESTNUT-TREE DISEASE IN MISSISSIPPI

PROFESSOR EUGENE HILGARD, of Berkeley, Cal., told me this summer of an observation of his which is of moment to those interested in the chestnut-bark disease.

While surveying in 1856 in the northeastern part of Mississippi, he found the chestnut trees of that region, both young and old, dead. They had been growing in a mixed forest of pine and oak and, as the other trees were in a healthy condition, were very noticeable. The dead trees were frequently of large proportions, attaining a height of 80 to 90 feet. When he saw them, these trees were beginning to decay; the bark was dropping off, leaving the trunks bare. There were no signs of insects. The region which was surveyed is a non-calcareous one.

As chestnuts are still growing in northeastern Mississippi, the epidemic which Professor Hilgard saw did not exterminate the tree in that region. It is another record of a devastating disease which the chestnut tree has endured.

Now that extra attention has been given to the chestnut and old records have been looked over, the struggle which this tree has had against attacks of fungi and of insects during the nineteenth century becomes apparent. There can hardly be a doubt but that the present range of this tree is much less extensive than formerly.

CAROLINE RUMBOLD

BLANDING'S TURTLE

TO THE EDITOR OF SCIENCE: Referring to Mr. Howe's note in SCIENCE of September 1, "Second Record for Blanding's Turtle in Concord, Mass.," and of the introduction of three pairs of the same species in Little Long Pond, Orange County, by Dr. Townsend, I beg leave to report finding this turtle at Queens, L. I., in June, 1909. It has been placed on the records of the Natural History Survey of Long Island now being made by the Brooklyn Institute of Arts and Sciences. This is the first report, so far as we know, of Blanding's turtle having been found on Long Island, but Abbott in "A Naturalist's Rambles about Home" mentions finding it in central New Jersey.

JOHN J. SCHOONHOVEN

THE MOTH OF THE COTTON WORM

TO THE EDITOR OF SCIENCE: In connection with the notices appearing in SCIENCE (October 16 and November 10) recording the occurrence far north of the moth of the cotton

worm (*Alabama argillacea* Hubn.), it may be worth while to place on record the fact that this insect has been very abundant in parts of the south this year. Here at least, and if one may judge from observations from a car window, in northern Alabama as well, the cotton has suffered also complete defoliation.

J. R. Watson
Florida Agricultural
Experiment Station

TRANSPLANTATION OF OVARIES

To the Editor of Science: May I have space in your columns to say a few final words regarding the results of transplantation of ovaries?[1]

Professor Castle has objected to my application of the term mongrel to guinea-pigs used by him in experiments which he claims overthrow my results on chickens.[2] My authority for the use of this term is the following extracts from his paper.[3]

The ovaries were removed from an albino guinea-pig and in their stead were placed two ovaries, one from each of two black guinea-pigs. The female bearing the engrafted ovaries was subsequently bred to an albino male and of the resulting six young, all were black and red, and one had a white foot. In explanation of this white foot, it is stated that "*Spotting characterized the race from which the father came. He was himself born in a litter which contained spotted young. . . .*"[4] Therefore the male was a mongrel.[5]

In the other instance, an albino female was spayed and her ovaries replaced by the ovaries of a brown-eyed cream guinea-pig. The albino female was then bred to an albino male and two albino and one brown-eyed cream offspring resulted. In attempting to explain this result, it is stated that "*albinism occurred as a recessive character in the particular brown-eyed cream stock used. . . .*"[4] So it follows that at least one of the females used in this experiment was a mongrel, and was therefore, as in the first experiment, entirely unsuited to furnish any reliable information from the standpoint of foster-mother influence.

C. C. Guthrie
Physiological Laboratory,
University of Pittsburgh

[1] Science, N. S., 1911, XXXIII.

[2] Science, N. S., 1911, XXXIII.

[3] Publication No. 144, Carnegie Institution, pp. 9–10.

[4] Italics mine.

[5] In an article by Professor Castle appearing in *The Popular Science Monthly* under date of May, 1910, it is stated that in such an experiment six young resulted and they were "*all black*" (italics mine). From the data in my hands it is impossible to conclude whether this is the same experiment as that quoted above, and to which it bears a striking similarity. If it is the identical experiment, and this I assume in view of his more recent statement (Publication No. 144, Carnegie Institution, 1911, p. 8) that but two of his successfully operated animals had borne young, the article in *The Popular Science Monthly* must be inaccurate.

MOULTING AND CHANGE OF COLOR OF COAT IN MICE

Mr. C. C. Little has, in a recent number of Science (October 27, 1911), taken exception to certain statements that I made in an article on the inheritance of coat colors in mice. He believes that the unusual patterns that I have described, especially in black mice, which I attributed in part to a heterogeneous condition, are only temporary effects and are due to moulting. That the coat may appear spotted at times of moulting is too familiar to any one keeping these animals to call for comment. But that the patterns that I described are not due to this was shown by the fact, stated in my paper, that the fully grown hair was in all cases studied under the microscope and the pigments in the hair recorded. Moreover, the cases described were not incidental to the coat-changing period, for the pattern remained for several months until, in fact, a new moult appeared.

It is well known that black mice contain both black and chocolate in the hair, even when they produce only black mice. Hence the opportunity is furnished for the local excess of one or of the other pigment to become apparent. That such effects are due to some "physiological conditions" present at the time of moulting is very probable, and was mentioned in my paper. Furthermore, in

a series of experiments in which black and chocolate mice were crossed through several generations, the spotting in the heterozygous mice—known to be such—was very prevalent. Finally, that even dilute colors are themselves modifiable by the condition of the animal when the next coat is formed was illustrated by some of the cases that I described, and is a phenomenon well known to breeders of animals. It is true that such cases do not show the animals to be heterozygous and therefore the presence of spots can not in itself be taken as a safe criterion of that condition. But my evidence showed that heterozygous mice frequently give evidence of their dual nature. In other cases also, as in the pomace fly, where I have found a dominant and a recessive character both present in the same individual, breeding tests have shown such individuals to be heterozygous.

T. H. Morgan

QUOTATIONS

THE ROYAL SOCIETY

At the anniversary meeting of the Royal Society yesterday afternoon the president made an announcement of unusual interest. On July 15 of next year the society will have been in existence for two centuries and a half; and it has been decided to celebrate the occasion in the manner prescribed by custom for such functions of retrospection and congratulation. For this particular function a new descriptive word seems necessary. It is not a jubilee, or a centenary, or a bicentenary, or a tercentenary, with all of which we have been made familiar, but something compounded of a bicentenary and a jubilee. Some compendious title seems to be required, but Sir Archibald Geikie managed to do without one, and what the Royal Society has been unable to invent it would be rash on the part of any other authority to supply. We must all be content to say that the Royal Society is going to celebrate the 250th anniversary of its foundation. The chief universities, academies, scientific societies and other institutions in this country, in the dominions and abroad are to be invited to send delegates to take part in the ceremony, of the importance of which the king, as patron of the society, has been pleased to express his appreciation. In view of the high place held by the Royal Society among the scientific institutions of the world, and of the eminent services which by universal consent it has rendered to science, there can be no doubt that the response to its invitation will be ample and generous. Next year will witness a large and brilliant gathering of men of science from every part of the civilized world, eager to testify to the respect which the long history of the Royal Society has inspired among all seekers after natural knowledge. Though the principles of the great quest are always the same, two and a half centuries bring many and profound changes in methods and conditions. Many ideas once cherished have to be dropped, and many new ones assimilated. Fundamental theories become outworn, and the most fruitful hypotheses, having served their purpose, have to give place to newer generalizations. The best proof of the vitality of the Royal Society is that it has survived all these transformations, and that it holds its place to-day, as in earlier years, in the van of the great army of students of the laws and structure of the universe.

Though the progress of science has been continuous through the long period covered by the lifetime of the Royal Society, the rate of progress has not been by any means uniform. The great intellectual upheaval of the renaissance gave a powerful impulse to scientific inquiry, after centuries of extremely slow progress. But that special impulse in turn exhausted its strength, and was followed by a period of smaller achievement. The end of the eighteenth century saw the beginning of another great era of activity, which continues to the present day in shapes that more and more conform to Bacon's contention that the pursuit of knowledge should be directed to the improvement of the conditions of human existence. Men now living have been witnesses of a great transformation, at least in

the external aspects of scientific activity. Superficially it might seem that science has lost something of its interest for the mass of the nation by the disappearance of the rather heated controversies in which men of science took part a generation or two ago. We have, for example, no controversialist like Huxley to arrest attention by a lively polemic connecting science with cherished beliefs in another sphere. But that is really evidence that science is better and more widely understood by the mass of the nation than it was in his day, and perhaps also that men of science themselves have advanced beyond a standpoint from which such a polemic appeared useful. It may even be noted that scientific thought is less concerned than it was with abstract disputation, and applies itself much more closely to more positive and practical lines of inquiry. The note of the present day is the enormous extension of applied science, and the danger is that the minute specialization such extension involves may militate against the appearance of one of the commanding intellects that from time to time have opened up a new world. It seems to some observers that some great step in advance is due for the whole scientific army, as distinguished from the mass of excellent detailed work now done upon existing lines. We have as it were a great scientific community working out the exploration of a region long ago discovered and surveyed, but there is room for some one who shall climb to the top of Pisgah, and announce to us a new land of promise which man may enter and possess. As we can not feed upon the crude elements that build up our bodies, but must depend upon plants as intermediaries, so in our manifold and voracious activities we are using up intermediate products of natural forces, the store of which is not inexhaustible, but we have not learned how to harness the natural forces themselves for our purposes—the energy of the sun, the power of the tides, and the yet unpenetrated processes by which nature, in the quietest manner, achieves results only imitated in our laboratories by enormous expenditure of stored-up energy.—London *Times*.

SCIENTIFIC BOOKS

The Voyage of the "Why Not?" in the Antarctic. The Journal of the Second French South Polar Expedition, 1908–10. By Dr. Jean Charcot. English version by Philip Walsh. Illustrated. 4to, pp. viii + 315. New York, Hodder and Stoughton.

This expedition, the second made by Dr. Charcot to the Antarctic, was not a south-polar quest, but was for scientific exploration. Fitted out by the French government at an expense of $140,000, it was aided by various subscriptions to the extent of $20,000 in money. Additional gifts and loans from learned institutions made "the scientific arsenal one of the richest and completest ever carried by a polar expedition."

> The exact object of the expedition was to study in detail, and from all points of view, as wide a stretch as possible of the Antarctic in this sector of the circle, regardless of latitude. I knew that I had chosen the region (south of Cape Horn) where ice confronts the navigator as far north as 61°, and where the coastline is fringed with high mountains, to all appearance insurmountable.

One phase connected with the expedition was unusual, illustrative as it was of that generous spirit of cooperation in scientific investigations, which to-day causes all civilized nations to interest themselves in ventures of general welfare. It was natural that French generosity should be manifest in donations for an expedition of its own government, but that other nationalities should tender material and important aid was as gratifying as it is unusual. Mr. Gordon Bennet with customary generosity filled the bunkers of the *Why Not?* at Madeira. The Prince of Monaco gave a complete oceanographical outfit. The meteorological department of the Argentine Republic loaned scientific instruments. Chili contributed seventy tons of coal. Brazil not only gave one hundred tons of coal on the outward passage, but also filled the bunkers on the return, both at Rio and at Pernambuco.

The admirable manner in which the ship did her work was due to the care, foresight and judgment exercised in planning and in building the *Why Not?*. The general equip-

ment and arrangements for scientific work were equally satisfactory, but the canned provisions either from character or from quality were unsuited to prevent scurvy. Nine of the twenty-nine members of the personnel were polar veterans, whose services were entirely satisfactory—producing a maximum of possible results.

Leaving Havre, August 15, 1908, the *Why Not?* sailed *via* Rio and Buenos Aires—in which cities great interest was shown and material aid given—to Punta Arenas, whence she departed on December 16. At Port Deception, South Shetlands, was found a steam-fleet—engaged in the renewed whaling enterprizes—from which Charcot obtained his last coal.

Favored by fine weather the *Why Not?* skirted the west coasts of Palmer and Graham Lands, making many discoveries and reaching Alexander Land. Obliged to return for winter quarters to Peterman Island, the ship grounded en route and barely escaped destruction.

After eight months in winter-quarters Charcot was able to break out, and obtaining coal at South Shetlands—to renew his explorations to the south in the summer of 1909–1910, when his success was phenomenal.

In the two summer voyages he extended this part of the continent of Antarctica from the Antarctic circle to 70° S., surveying Loubet coast, discovering and mapping Fallières Land, extending Adelaide Island from an islet to a land seventy miles long, opening Marguerite Bay, surveying Alexander I. Land, and finally discovering Charcot Land in 77° W., 70° S., a mountainous, almost ice-covered region—doubtless a part of the continent.

Keeping to the west the *Why Not?* traversed unknown areas, along the parallel of 70° S., from 103° W. to 124° W. (except on the 107th meridian where Cook passed); in latitudes from two to three hundred miles to the south of Charcot's predecessors—he sounding as he sailed.

The second voyage was made under conditions of great peril, for a survey of the *Why Not?* by a diver at South Shetlands disclosed that "The whole stem below water-line was torn away, as well as several meters of the keel: the slightest shock might send the ship to the bottom." The diver remonstrated, yet Charcot sailed.

This being a popular volume, it does not give the results of the immense amount of scientific work done, including observations on gravity, seismology, meteorology, geology, tides, magnetism, zoology and oceanography. Many attractive sidelights are, however, thrown on these subjects by the notes made from day to day. A spirit of French gaiety and good humor pervades the book, and these qualities were evidently characteristic of the party as a whole.

The generous spirit shown by Dr. Charcot in giving due credit to his predecessors adds much to the enjoyment of his narrative. Such action is in striking contrast to the unfortunate tendency of some explorers of smaller mind to mar the value of their own exploits through neglect or by disparagement of the work of others, whether associates or rivals. Especially grateful to Americans are the credits given and justice done to Palmer and Pendleton.

The volume is most creditable to the publishers, and the translation good. The illustrations are excellent, but the south-polar chart should have been on a larger scale, with side maps, and its text should have been in English. The volume will interest all readers fond of travel and exploration.

A. W. GREELY

Stereoscopisches Sehen und Messen. By von CARL PULLFRICH. Jena: Gustav Fischer. 1911. Pp. 40.

This useful pamphlet, so far as the text is concerned, is available in English as the article on the "Stereoscope" in the eleventh edition of the Encyclopedia Britannica. The pamphlet contains some supplementary statements; but its notable addition is a bibliography of 276 numbers covering the period 1900–1911. This in turn supplements the bibliography available in M. von Rohr: "Die

binokenlaren Instrumente." Both text and bibliography are concerned predominantly and designedly with the physical problems of stereoscopy, though the discussion of the resulting refinements and variations of the stereoscopic effect reflects indirectly upon the psychological problems. While involving at each step questions of psychological analysis and theory, the essential advances in stereoscopy have been physical in nature. In part they constitute the physical solutions of problems raised by the study of depth-perception; and in yet larger measure they constitute original physical problems of application, extension, and quantitative refinement of the stereoscopic principle. However, the existence of an adequate psychological study contributes to the physical problems a very different status than attached to them fifty years ago, when it was difficult to convince the scientific public that psychology had any logical right or proprietary interest in an instrument made of prisms, or lenses, or mirrors.

The renascence of interest in stereoscopic problems is abundantly evidenced by the extent of the literature, and further by the great variety of publications in physiological, psychological, ophthalmological and general scientific journals, together with applications of stereoscopic presentations to scientific, educational and technical procedure. While the expository article of Dr. Pullfrich touches upon but few of these phases of the subject, it is written with a background reference to them, as a support of the interest in the problems considered. It seems the irony of fate that the man who by construction and analysis had done so much to make possible the refinements of stereoscopic vision, is himself deprived of their enjoyment. Having lost the use of one eye, Dr. Pullfrich records that to him the beauties of stereoscopic effects are a matter of remembrance only.

Pullfrich's exposition is itself so condensed that this notice may be confined to an account of its method and procedure. The fundamental condition of stereoscopic vision is the separation and position of the eyes in the head, the variations of which in different animals offer suggestive and as yet incompletely interpreted potentialities of depth-perception. The part of the visual field in which stereoscopic vision operates is limited, and makes the reports of space-relations from those portions of the visible world which the right eye and the left eye respectively but exclusively survey, a special problem of *indirect* stereoscopy. Obviously such report is momentary and shifting, since a turn of the eyes brings the outlying object into the binocularly policed territory. As to the physiological or psychological basis of the team-work which the two eyes so marvelously perform, we are reduced to ingenious hypotheses. The principles of stereoscopic vision express merely the conditions of conformity necessary to the production of the depth-effect, and the corollaries of variation in effect resulting from shifting values of the many variable contributors. It has become clear that the presumable alternative of the earlier discussions between the part played by "retinal dissimilarity" and by "convergence shiftings," is not an exclusive one; the two jointly contribute to the effect in practise, and this circumstance reflects back upon the theory, by suggesting that tentative motor initiatives may even fuse with seemingly instantaneous retinal impressions.

The physical problems are, in a sense, conditioned by the marvelous precision of the psychological depth-perceiving mechanism; for were not the optical instrument supported by the visual fineness of distinction, there would be no possibility of the utilization by the eyes of the extensions and controls of its verdicts which the inventions of Dr. Pullfrich and the constructions of the firm of Zeiss have added to the triumphs of science. The problems arising from the reconstruction of a natural depth-effect from the combination of two photographic (or diagrammatic) views—the divergence of which reproduces the difference resulting from the base line of the interocular distance —are naturally distinct from those growing out of the project of extending the range or degree of depth-perception of an actual and

extensive three-dimensional world. The former problem was in principle solved by Wheatstone, and its perfection in securing an orthostereoscopic effect—apart from convenience and refinement—follows upon the analyses and elimination of the incidental and unintentional deviations between the optical system of the photographic reproduction, and that of the original visual experience. Invention has been fertile, especially in devices for presenting to the eyes the two divergent views, leading to such diverse pieces of viewing apparatus as the reflecting stereoscope of Wheatstone, the refracting one of Brewster, the lenticular of Helmholtz, the complementary chromatic effect of Rollman-d'Almeida, the Ives parallax stereogram, the unilateral reflecting stereoscope of Pigeon, and in another direction, to the invention of the Verant lenses; in yet another, to the devices for stereoscopic projection, and again to the study of pseudostereoscopy. The enlargement and precision of stereoscopic vision has led to the stereotelemeter, in which the projection of a scale incorporated in the optical system of the instrument (by engraved lines on the objective, or equivalent device) over the distant landscape gives accurate stereoscopic judgments at a telescopic range. Conversely the stereo-comparator provides the means of restoring to space-relations of the third dimension, the minute transverse deviations of the two divergent representations resulting from any given real (or calculated) base line. From this, in turn, other problems diverge, such as that of constructing an equally precise photographic stereo-camera, and again of restoring from the stereograms thus resulting, the actual object—say, a statue—in its three dimensional reality. No less accurately than a phonographic disc preserves a voice for posterity may a solid reproduction of our actual bodily self in length, breadth, thickness of build and feature, be embodied on the twin record of a true stereoscopic print. Finally, of applications of stereoscopic principles there are many and varied examples, from the detection of forgeries to that of the variability of stars, or examination of microscopic specimens.

Pullfrich's article is devoted not to the description or analytic aspects of the problems of which these several inventions form the solutions, but to the clear and concise statement of the physical (and mathematical) aspects of the constructions involved, with due reference to the functional service sought. For this specific purpose, as well as for a general survey of the recent advances in stereoscopy, the pamphlet may be unreservedly recommended.

JOSEPH JASTROW

De Fabricatie van Suiker uit Suikerriet op Iava. By H. C. PRINSEN GEERLIGS. Amsterdam, J. H. De Bussy. 1911. Second edition. Pp. xxiv + 500 + xx.

Desire to keep pace with the rapid advances which the art of sugar-making is constantly experiencing has induced Prinsen Geerligs, the well-known Dutch sugar-expert and author, to prepare this new edition of his book, which was originally published in 1907.

The work is divided into three sections. The first of these is given over to a consideration of the raw material and discusses the occurrence and distribution of the various constituents of the sugar-cane—sucrose, dextrose, lævulose, invert-sugar, fiber, the pectins, organic acids, cane-wax, coloring matters, nitrogenous bodies and mineral matters.

The second section is concerned with the technology of sugar-making. Attention is first given to the extraction of the juice from the cane and in this connection there are considered, sugar-mills, processes of diffusion, the treatment of bagasse and determination of its fuel-value. Then follows an exhaustive discussion of various processes of defecation and carbonatation, having for their object the clarifying of the crude cane-juice, and a detailed review of various reagents employed for the purpose.

Under the caption "Concentration of the Juice," the author deals with the preliminary concentration of sugar solutions, vacuum-pans and their accessories, and the working up of by-products.

As is well warranted by its importance, a separate chapter is devoted to the composition and utilization of molasses; Java-molasses, to the study of which the author has given much time and personal attention, receives specific consideration.

The final section of the book deals with factory-output, calculations and records; extensive tables and a satisfactory index conclude the volume.

This brief outline of the book's contents will indicate in how thorough and painstaking a manner the author has acquitted himself of his self-appointed task. His familiarity with the work of other investigators, with that of his American confrères among others, is amply attested by foot-notes and references scattered throughout the volume.

The straightforward, lucid style in which this book is written is characteristic of its author and makes its reading a pleasure, nor must the excellent make-up of the publication pass unnoticed—the quality of paper used, its typography, the marginal indices, all certainly merit the appreciation of its readers.

F. G. Wiechmann

An Introduction to the Lie Theory of One-Parameter Groups, with applications to the solution of differential equations. By Abraham Cohen, Ph.D., Associate in Mathematics, Johns Hopkins University. Boston, D. C. Heath & Co. 1911. Pp. iv + 248. Half leather.

The scope of this attractive little volume may be inferred from its seven chapter headings, which are as follows: Lie's theory of one-parameter groups, differential equations of the first order, miscellaneous theorems and geometrical applications, differential equations of the second and higher orders, linear partial differential equations of the first order, ordinary differential equations of the second order and contact transformations.

In form, binding and paper the present volume is similar to the "Elementary Treatise on Differential Equations," by the same author, published in 1906. In subject matter it forms a suitable sequel to this work, but it can be read with a more limited knowledge of differential equations. While it should appeal especially to the student of mathematics who is about to begin graduate work in an American university, it should also prove useful to those who make frequent use of the differential equation in applied fields of mathematics and who desire to look at the subject from the systematizing and clarifying standpoint of group theory.

The book closes with an appendix containing seven notes, two tables, answers to the examples, and a good index. In these notes several important subjects are developed for which there was no room in the body of the book. In particular, the n-parameter group of transformations is considered briefly in one of these notes. The two tables contain forms of differential equations of the first and of higher order which are invariant under known groups.

It is very gratifying to witness the rapid increase of American mathematical literature suitable for students who are just beginning graduate work. Even very good students of mathematics have found the transition period from undergraduate to graduate work discouragingly difficult because they were all at once compelled to use foreign literature with an abrupt change of point of view and method of presentation. During the last decade much has been done to remedy this serious drawback, but there are still many lacunas in this literature. The present volume has reduced by one the number of the most important of these.

G. A. Miller

SPECIAL ARTICLES

CARBON DIOXIDE AT HIGH PRESSURE AND THE ARTIFICIAL RIPENING OF PERSIMMONS

It is already known from the work of Prinsen-Geerligs (through Gore, 1910) on the fruit of the banana that its astringency disappears, without softening of the pulp (mesocarp), when surrounded by an atmosphere deprived of oxygen. This result suggested to

Gore, of the Bureau of Chemistry, U. S. Department of Agriculture, that the use of any "inert" gas would be equally efficient, and accordingly he tried the effect of carbon dioxide (Gore, 1910, 1911). The outcome of his experiments, as of my own (Lloyd, 1911) is to show that the results of Japanese method of processing, a procedure, apparently empirical, of exposing the fruit to the fumes of sake by packing in recently emptied casks, may be duplicated. Indeed, according to Fairchild (1905, 1911), attempts on a limited scale to imitate the Japanese method in this country, had already met with success, so that there could have been little doubt in the minds of those cognizant of the facts that, with effort, but a short time need intervene before a method could be worked out for attaining, even on a practical scale, the results desired. The expectation must have been strengthened by the results obtained by Vinson (1909) on the date (*Phœnix dactylifera*) whose fruit possesses, as regards the tannin cells, cytological characters precisely similar to those of the persimmon, sapodilla and other, doubtless numerous, fruits. Vinson found that acetic acid fumes can be used for processing dates to prepare them for the market, the immediate and important outcome being the loss of astringency. He (Vinson, 1910) further found that a host of reagents of the same phase can be used, but with attendant results undesirable from an economic point of view. Of these, for a single example, nitrous ether affects the tannin cells in such a manner that the contained tannin is no longer free to enter into solution, and hence the non-astringency. Even heat may be used to hasten the result, as Vinson (1907, 1910, 1911) also showed, while Freeman (1911) extended the use of this discovery in a practical way.

In order to describe the loss of astringency, it was stated jointly by Bigelow, Gore and Howard (1906) and later by Vinson (1907) that the tannin in the persimmon becomes "insoluble" during the ripening process. But, as I have shown, "insoluble" tannin is not otherwise known. To use the adjective quoted is to contravene the accepted definitions of the substance therewith described. Indeed, the astonishing unusualness of the conception appears when it is realized that there is known no compound of tannin with organic products, composing "leather" of one form or another, which, by repeated contact with fresh solvent, will not give up fractions of the amount of tannin originally bound up with the associated substance. Analogies in the field of colloidal chemistry will immediately occur to those even only slightly initiated into its mysteries, for such they surely are at the present moment. My own work begun on the date (1910) in 1907 and continued on the persimmon (1911, *a* and *b*) and sapodilla (1911, *b*) has enabled me to throw some light on the difficulty. I have shown that, during the process of ripening, the loss of astringency is due to the union of the tannin with an associated colloid (1911, *a*) of carbohydrate nature (1911, *b*), which is an intraprotoplastic product in common with the tannin itself, and which undergoes a gradual coagulation as seen in its increasing firmness and loss of swelling capacity. During this process the tannin becomes adsorbed, and there is thus formed a vegetable leather, from which only exceedingly small fractions may be extracted by ordinary solvents (water, alcohol), but which may be attacked chemically and then extracted by means of strong nitric acid, leaving the entire, or nearly the entire amount of the associated colloid intact (Lloyd, 1911, *c*). We may thus obtain this as yet practically unknown body for examination by chemical methods, which have already yielded some important results, indicating as above noted its carbohydrate nature.

So that it develops that the essential fact of the loss of astringency is the formation of a colloidal complex, of which tannin is one, and another carbohydrate the other member. Disregarding those reagents which have a chemical effect upon the tannin itself (so far as we can determine this) there is a number of substances which are capable of hastening this process. Alcohol and acetic acid vapors (the possible chemical effect of the latter on

tannin being disregarded for the moment) do so, but the change is accompanied by other autolytic processes, in equal rates, which cause the digestion of the middle lamella, and, probably, conversion of sugars and of aromatic substances. Carbon dioxide, however, is peculiar in that the loss of astringency is materially hastened while, relatively, the remaining changes are held in abeyance, being, however, hastened as compared with normal conditions. Herein lies the secret of the Japanese process, the source of the carbon dioxide being the fruit itself, as a result of respiration. As Gore (1910, 1911) showed, a pure atmosphere of carbon dioxide, making available a larger amount, may be used. I have repeated his experiments with the goal of immediate practicality in view, and have succeeded in perfectly processing two varieties of Japanese persimmons (*Taber 129* and *Hyakume*) in churn barrels on a scale sufficiently large to demonstrate the feasibility and low cost of the method for the grower or merchant. The mechanical features of the churn barrel (of the "Daisy" churn) permit the easy packing of the fruit in suitable material and the lid may be made sufficiently tight to imprison the carbon dioxide for the period necessary without recharging. The details of the method will appear in another form later. But this is not all, nor the most important feature of this work. I have further demonstrated that, *under increased pressure of carbon dioxid, the processing is hastened*, so that, with a pressure of 15 pounds the time required may be reduced from six to seven days, the time required under normal pressure, to less than two days. The definiteness of the experiment on this score leaves so little to be desired that I venture to detail it. Six dozen fruits, of the two varieties mentioned in nearly equal numbers, were introduced into an autoclave (The Eclipse Sterilizer) at 4 P.M., November 16. No packing was used, the fruits being placed side by side on cardboard trays. The air in the autoclave was first displaced, after which the pressure was increased to 15 pounds. A somewhat sudden lowering of the pressure from 15 to 14 pounds was attributed to the penetration of the gas into the fruit, and, at the expiration of 20 minutes, the pressure was again raised to 15 pounds. During the night, however, the pressure fell to normal, and, after recharging, the safety valve was found to leak. After adjustment the pressure remained close to 15 pounds. At 5 P.M. the same day, the pressure was raised to 18 pounds, but it fell during the night to 17 pounds, falling to 15 pounds by 2 P.M. The autoclave was then opened, and the fruit tested. The fruits examined, in all about three dozen, by various persons, were found completely processed, save that, up to date, one particularly hard and light-colored fruit was found very slightly astringent. By way of further practical test, a dish of the fruit was served to a number of guests at my home on the night of the same day, and they all found the fruit very hard, sweet and delightfully aromatic.

Fruits of the same lot, but processed two weeks previously, yielded to the treatment under normal pressure in seven to eight days. The control experiment, in which fruits of the same lot were exposed to normal pressure in a churn barrel, yielded in 8 to 10 days.

It should not be overlooked that this surprisingly short period of two days may be too brief for fruit which has just been harvested, and may vary also with the time at which the harvesting takes place. In any event, however, as the control shows, the time involved will be materially shortened with increase of pressure in the amount already indicated.

The above experience leads us to infer that, if the initial pressure had been constantly maintained, the completion of the process might have been still further hastened, while it is possible that still higher pressures may be correspondingly more efficient. Setting this aside, since there are a number of questions regarding color, degree of maturity and the like, in their relations to the rate and agents of processing which must be further studied, there is no doubt that the method of making use of supranormal pressures of carbon dioxid may be employed to an advantage

superior to that of normal pressure. The somewhat increased cost necessitated by a tight receiver is more than offset by the reduced time involved, by the certainty of the results and by the immediate availability. A suitable receiver of heavy galvanized iron similar to a kind already on the market, can be manufactured at a reasonable cost.

But the practical value of this result is no greater than its theoretical bearing. It appears, in the first place, that the rôle of carbon dioxid is not associated with its inertness, but is, on the contrary, positive, a conclusion demanded, I believe, by the time relations. How this view may be harmonized by the remaining heterogeneous mass of facts, at present available through the efforts of the above-mentioned workers, it is too early to say, but it will prove, I believe, a fruitful suggestion that the explanations demanded are to be found in the relation of the heterogeneous reagents to the colloids, and therefore in the colloidal-chemical reactions, rather than in the better understood chemical relations. One may not overlook the capital fact that other of the ripening processes are hastened by carbon dioxid, but in different degrees, suggesting the effect of a general catalytic agent. An exception, however, is to be noted in the cessation of color changes. A fruit, which is yellow when subjected to carbon dioxid, does not subsequently change to the usual deep orange of the normally ripened.

From my study of the tannin cells themselves it emerges that the increase in rigidity of the tannin-masses, a slow process under ordinary conditions, is hastened under normal, and still more under supranormal pressures of carbon dioxid, but is *preceded,* by a relatively brief period, *by the completion of non-astringency.* From this it may, for the present, be inferred that the disappearance of soluble tannin is connected with the coagulation of the associated colloid, and that the rôle of the acid, carbon dioxid, is, directly or indirectly, the cause of this coagulation which proceeds up to some, at present, unknown limit at a rate related to the amount of acid available. That, by coagulation, the physico-chemical condition of the colloid, and its consequent behavior toward tannin, may be changed finds an analogy, perhaps not too loose for my purpose, in the behavior of the micropylar colloid stopping the micropyle in the egg of *Fundulus.* According to the view of Jacques Loeb (1911) this colloid becomes "tanned" in the course of one or two days if in the surrounding water certain salts are present, thereby rendering the micropylar plug semi-permeable to sodium chloride, and so preventing the toxic effect of this salt upon the embryo within.

But whatever the importance of the explanation of the phenomena in the moribund fruit, the physiological meaning of the associated colloid during its period of development is certainly not of less. It is known that the coagulation of casein by HCl may be prevented by the association, with the casein, of another, a "protective," colloid (Alexander, 1910). On the strength of this fact, Alexander has been able to throw an important light on the digestibility of human, as compared with bovine and certain other milks. It seems not improbable that, in the growing cell, such is the relation of its associated colloid to the tannin, thus preventing its attack upon the protoplasm. This, as a working theory, has a not inconsiderable tentative value. In harmony therewith is the fact that the tannin in the persimmon, as in the date, always remains within the cell, the tannin-idioplast, in which it originates (Lloyd, 1910, 1911).

Concerning the action of heat, which, it has already been said, causes coagulation of the associated colloid, Vinson, cited above, has shown that too high temperatures, sufficient to destroy enzymes, prevent normal ripening (that is, as related to astringency), while suitable temperatures, yet fatal to protoplasm, hasten it. He sees in these facts evidence of the presence of enzymes. I have shown that high temperatures (that of the boiling of concentrated cane-sugar solution) actually coagulate the associated colloid (1911), but without the complete imprisonment within it of the tannin. It seems clear from this that the time

relation is an important one, and that, during normal ripening, an enzymatic agent is at work effecting the coagulation. If this be true, the rôle of carbon dioxid may be less direct than above indicated, and that its business lies in hastening the secretion or the activity of the responsible enzyme.

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FUNDULUS AND FRESH WATER

IN a series of papers[1] published in 1906 and 1907, I presented the results of experiments in which fishes of a number of species (particularly *Fundulus heteroclitus*) had been subjected to various modifications of the salt content of the containing water and to various other abnormal conditions. Contrary to the previously published statements of Loeb[2] and of Garrey,[3] I found that in the great majority

[1] *Biological Bulletin*, May, 1906; Bulletin of the Bureau of Fisheries for 1905 (May, 1906); *American Journal of Physiology*, June, 1907. Further data were reported in a paper before the seventh International Zoological Congress in 1907 (to be published).

[2] *American Journal of Physiology*, Vol. 3, 1900, pp. 327–338. I regret to say that my criticism of this writer was framed in language which, though not intended offensively, I now recognize to have been in poor taste.

[3] *Biological Bulletin*, Vol. VIII., 1905, pp. 257–270.

of cases *Fundulus heteroclitus* died, either in distilled water or in ordinary fresh water, drawn from the water supply of New York or of Woods Hole. In my own experiments with healthy adult fishes, placed in ordinary "tap" water, death commonly occurred after an interval of from one day to two weeks, although individuals frequently lived for a considerably longer period, sometimes as long as they were kept under observation. In some of the experiments the earliest deaths occurred too soon to make it possible to attribute them to bacterial or fungous disease, while in the great majority of fishes there was no visible evidence of such disease to the last.

I have been very careful to avoid making the claim that *Fundulus* of this species could not, *under any circumstances,* live for an indefinite period, either in fresh or distilled water. Indeed, as regards the former, I cited trustworthy reports of cases in which this fish had become landlocked in ponds, etc., probably through a slow process of acclimatization. I have, however, laid emphasis upon the fact that adult specimens *do commonly* die within a few days after transfer to water entirely devoid of their accustomed salts. From recent conversations with Professor Loeb, I am led to understand that this has likewise been his own experience.[4] Indeed, in a paper[5] published during the present year he distinctly affirms that only five per cent. of his grown individuals show sufficient powers of resistance ("diese grosse Widerstandsfähigheit") to live for five weeks in distilled water. On this point, then, the difference between us seems to be merely a matter of emphasis. Loeb, for his purposes, has laid stress upon those cases in which the fishes have survived; I have laid stress upon the fact that, except for brief periods, they commonly do not survive.

In itself, it would seem to be a matter of small scientific importance whether or not any given species of fish can be transferred with impunity from one medium to another. We all know that some fishes can, while many can not, endure such a transfer. But since the experiments, both of Loeb and myself, in this field, have dealt very largely with the question whether or not this particular species would survive various experimental conditions which have been employed by us, it is of considerable importance to recognize its *ordinary* behavior in fresh water.

In a recent article[6] already referred to, Loeb has made much of the fact that I admittedly used commercial distilled water in my experiments, and would clearly have his readers believe that the death of the fishes in these experiments was due to impurities in the water. It seems hardly necessary for me to state that my use of water of this sort was deliberate and was done with a full knowledge of the fact that ordinary distilled water has been found harmful to some organisms. I used this sort of water for the simple reason that I was not, at the time, in a position to obtain sufficient quantities of chemically pure distilled water. I believe, however, that the validity of my results was not affected by the character of the distilled water employed, and this I hold for several reasons: (1) These fishes likewise died in ordinary "tap" water, in which true fresh water species lived perfectly well. Loeb's suggestion that disease germs may have caused the deaths in such cases is negatived by the fact that death oftentimes occurred within less than a day. (2) The baneful effects both of the distilled water and of the tap-water were abolished by the introduction of a very small percentage either of sea-water or of pure NaCl, as will be pointed out below.[7] (3) I must repeat Loeb's own ad-

[4] If I am mistaken in this, I trust that Professor Loeb will set me right.

[5] *Archiv für Entwicklungsmechanik,* Bd. 31, 1911, pp. 654–657.

[6] *Archiv für Entwicklungsmechanik* (*loc. cit.*).

[7] Here an appeal may be made to the antagonistic effect (discussed below) of the salts of sea-water upon various poisons, it being assumed that the distilled water had been contaminated by some metallic poison. Such an assumption could not be made, however, in the case of the tap water used, while the action of the NaCl in the two cases seems to have been identical.

mission, contained in the same paper in which he criticizes me, that all but five per cent. of the adult *Fundulus* died in his "double distilled" water. Why, then, assume that the water employed by me contained poisonous impurities? (4) Weighty evidence upon this same question is already afforded by an experiment which I have only recently commenced. At the date of writing this paragraph (Dec. 12), I have kept in distilled water for seven days eighteen specimens of *Fundulus heteroclitus,* taken from a fresh water stream and consequently habituated to the latter medium. The distilled water was prepared in an ordinary metal still. The fishes nevertheless all appear to be in perfect health, and no deaths have occurred in the lot since the first day, when two fishes died from causes having no bearing upon the present problem. This result is significant in comparison with that which had been obtained when salt-water specimens were used. The final outcome will be reported upon later.

As bearing on the question of the "protective" action of various salts, it may be relevant for me to point out that in 1906 I described experiments demonstrating the remarkable efficacy of even small percentages of sea-water in counteracting the fatal effects of fresh water upon *Fundulus.* I later showed[8] that this proportion need not exceed one part of sea-water to a hundred of ordinary fresh water. Experiments in which pure NaCl was employed, dissolved both in distilled water and in fresh ("tap") water, showed that this salt, in concentrations of 3 to 15 grams per liter (in some cases three tenths gram per liter), may preserve the lives of the fishes for three or four weeks or longer. Fishes kept in pure tap water, under otherwise similar conditions, all died within comparatively few days.

The fact that this fish will *endure* pure NaCl, in "very weak solutions," is now fully admitted by Loeb himself,[9] but he still appears to overlook the fact that, in such low concentrations, the salt in question is far from being a poison, but frequently preserves the fish from destruction.

Furthermore, we surely can not regard 15 grams per liter (a concentration tolerated by many of my fishes) as a "very weak solution." Indeed, it is roughly an $M/4$ solution, or one of more than half the concentration in which this salt occurs in sea-water.[10] Experiments in which sodium chloride was used in about the same concentration as in sea-water resulted in the death of all the (30) fishes used in from two to fifteen days.

In summing up this part of the discussion, I can but repeat my earlier statement that "In addition to such a toxic effect, however, the sodium chloride certainly has a potent *anti*-toxic effect, since, even in solutions which proved fatal, the rate of death was usually much lower than in pure fresh water. *In the aggregate, these experiments may be held to prove, therefore, that pure sodium chloride, in certain proportions, has nearly (if not quite) the same efficacy in counteracting the fatal influence of fresh water upon Fundulus heteroclitus as does the combination of salts contained in sea-water.* My previous experiments have abundantly proved, I think, that the action of this salt is not an osmotic but a chemical one" (1907, p. 73).

In a section of considerable length, entitled "The Toxicity of Certain Poisons as Affected by the Salinity and Osmotic Pressure of the Medium," I pointed out, among other things, that certain metallic salts (*e. g.,* copper chloride and sulphate, and mercuric chloride)

[8] *American Journal of Physiology* (*loc. cit.,* particularly pp. 68, 72, 73).

[9] *Archiv für Entwicklungsmechanik* (*loc. cit.*); also SCIENCE, November 17, 1911.

[10] Referring to some of his experiments with young *Fundulus,* Loeb tells us (SCIENCE, *loc. cit.*): "I succeeded in showing that as long as the sodium-chloride solution is very dilute and does not exceed the concentration of $M/8$, the addition of KCl and $CaCl_2$ is not required. Only when the solution of NaCl has a concentration above $M/8$ does it become harmful and does it require the addition of KCl and $CaCl_2$." The difference between Loeb's results and my own—of which last Loeb does not seem to be aware—may be due to the difference in the age of the fishes employed.

were more toxic in fresh water than in certain strengths of salt water, and this *even to fresh-water fishes.* One obvious interpretation is that these poisons were merely neutralized chemically by the ingredients of the sea-water, outside of the body of the fish, but this explanation is rendered improbable by a variety of considerations which can not be discussed within the limits of the present article.

The employment of pure NaCl, instead of sea-water, in these last experiments, would not probably have affected the outcome, if we may judge by recent work of Loeb, in which he found that the poisonous effect of zinc sulphate upon *Fundulus* eggs was neutralized by the former salt.

Loeb's assertion that "salts alone have such antagonistic effects" certainly does not apply to adult fishes. I need only call attention to the fact that cane-sugar solutions of certain strengths were found by me to very clearly defer the fatal action of the copper salts, both upon *Fundulus heteroclitus* and upon certain fresh-water species. It had first been ascertained that cane sugar did not, in any concentration, take the place of sea salts or of sodium chloride in prolonging indefinitely the life of *Fundulus.* Whether or not these facts can be brought into harmony with Loeb's "tanning" hypothesis, I do not pretend to know.

And now, while I am unearthing some of these long-buried records of the past, I can not refrain from repeating one of my articles of faith therein expressed:

> The writer is not in the least in sympathy with the tendency, so often manifested, to explain the most complex of natural phenomena by a few simple chemical or physical formulæ. If the principles which I have invoked [referring to certain tentative hypotheses] operate at all in the way in which I have supposed, they operate in conjunction with other principles so obscure and complex that a complete solution of these problems is certainly very far distant.

FRANCIS B. SUMNER

U. S. BUREAU OF FISHERIES,
WASHINGTON, D. C.,
November 28, 1911

SOCIETIES AND ACADEMIES

THE BOTANICAL SOCIETY OF WASHINGTON

THE 74th regular meeting of the society was held at the Cosmos Club, Tuesday, October 10, 1911, at eight o'clock P.M. In the absence of the regular officers, Dr. Albert Mann presided. Twenty-five members were present.

The following papers were read:

The Wilting Coefficient for Different Plants and its Indirect Determination: Dr. L. J. BRIGGS and Dr. H. L. SHANTZ. (Presented by Dr. Shantz.)

The Forest of Arden, a Dream: H. C. SKEELS.

The Forest of Arden is a 300-acre tract of native woodland, three miles east of Joliet, Ill., in the valley of Hickory Creek, and forms a part of the 2,000-acre estate, Harlow-Arden, of Mr. H. N. Higinbotham, of Chicago. The creek is dammed in three places, with locks through the two upper dams, giving a mile and a half of boating. Five miles of gravel drives have been laid out, the purpose being to display the landscape beauties of mixed meadows and woods to the best advantage. Along these drives, beginning with the ferns and following the accepted sequence of plant families to the composites, there has been planted a botanic garden of 2,000 species, room being left for as many more.

Each species is located by its place in the sequence, and by a map, cross-sectioned to square 100 feet on each side, accompanied by an index giving the plant names and the number of the square on which each will be found. There are no formal beds and no labels, but the species are there, to be seen by those interested.

The eleventh annual business meeting of the society was held on Tuesday, October 24, 1911. Officers were elected as follows: *President,* W. A. Orton; *Vice-president,* A. S. Hitchcock; *Recording Secretary,* Edw. C. Johnson; *Corresponding Secretary,* W. W. Stockberger; *Treasurer,* F. L. Lewton. The executive committee reported an active membership of 104, there having been nineteen accessions during the year.

The 75th regular meeting of the society, held November 14, 1911, in conjunction with the Washington Academy of Sciences, was devoted to a lecture by Dr. W. L. Johannsen, of Copenhagen. The subject of the lecture was "Heterozygosis in Pure Lines of Beans and Barley."

The 76th regular meeting was held at the Cos-

mos Club, Tuesday, December 5, 1911, at eight o'clock. President W. A. Orton presided. Thirty-three members were present.

The following papers were read: :

Thrips as Pollinators of Beets: Harry B. Shaw.

Thrips tabaci were observed to be numerous on seed beets in Utah. They were always abundant on flowering racemes, as many as 190 being collected from one small branched raceme. They were not observed to interfere with seed production. On the contrary, it appeared more probable that they acted as agents of pollination. An examination showed them to bear numerous pollen grains scattered about their bodies, as many as 140 beet pollen grains being counted on one adult thrips. An experiment, started August 7 and 8, 1911, under carefully arranged isolation conditions on emasculated beet flowers, resulted in 17.2 per cent. of the flowers to which thrips had been introduced being fertilized and producing seed. All the controls remained sterile. The conclusions are that thrips are probably important beet pollinators; that they may act similarly with other plants; that their absence or too small number may account for the non-fertilization of flowers in some localities and seasons; that they may fertilize flowers under supposedly isolated conditions and may even cross plants not regarded as capable of being crossed by insects, *e. g.*, barley; and that they may also spread fungus spores and bacteria.

Forest Types: Raphael Zon.

A study of Idaho forest types revealed three main factors: (1) yellow pine-Douglas fir, (2) cedar-hemlock, both climax types, and (3) pine-larch, a transitory type. The first formation is both a pioneer and climax type; the second is a climax type preceded by the transition type, the order of succession being first the larch (*Larix occidentalis*), then the white pine (*Pinus monticola*), and lastly the cedar (*Thuja plicata*), hemlock (*Tsuga heterophylla*), and white fir (*Abies concolor*).

Phytochemical Studies on Cyanogen: Dr. C. L. Alsberg and O. F. Black (by invitation).

W. W. Stockberger,
Corresponding Secretary

THE TORREY BOTANICAL CLUB

The meeting of October 10, 1911, was held at the American Museum of Natural History at 8:15 P.M., President Rusby presiding. Forty persons were present.

The minutes of the meetings of May 8 and May 31 were read and approved. Professor R. A. Harper, Columbia University; Dr. C. W. Ballard, 115 W. 68th Street; F. D. Fromme, Columbia University; A. B. Stout, New York Botanical Garden, and Miss C. Rabinowitz, New York City, were then proposed for membership.

The report of the secretary on the method of changing the day of a regular meeting was accepted. Dr. E. B. Southwick, chairman of the field committee, reported progress. A similar report was offered by Dr. Rusby, acting for the committee to revise the constitution.

Professor R. A. Harper, Dr. C. W. Ballard, F. D. Fromme, A. B. Stout and Miss C. Rabinowitz were elected to membership.

The scientific program consisted of a lecture on "Some Edible and Poisonous Mushrooms," by Dr. W. A. Murrill. The lecture was illustrated with lantern slides which had been made from photographs of specimens recently collected in the vicinity of New York City and colored while the specimens were in a fresh condition, thus enabling the artists to reproduce the natural coloration of the specimens photographed. The speaker stated that the exceptionally large number of recent deaths due to poisonous species of mushrooms was no doubt attributable to the abundant crops of *Amanita phalloides* and *Amanita muscaria* which have followed the copious rainfall of this season. Slides showing the poisonous species in several stages of growth were exhibited and the special marks of identification were pointed out. Following these were shown slides of some of the edible mushrooms easily confused with the poisonous varieties. The two most characteristic features of the poisonous mushroom are the "death cup" or volvas and the "ring" or annulus. The careless mushroom hunter may pull up a specimen, leaving the volva still buried in the earth, or the annulus, which is a more or less fragile structure, may have already disappeared, and serious consequences result from the oversight.

Dr. Murrill wished to emphasize the fact that there were no rules or tests that could be applied with certainty. It is necessary that one gathering mushrooms for eating purposes should confine his operations to such species as he knows intimately in all their various forms.

The lecture was discussed by Dr. H. H. Rusby, Dr. Thomas, E. B. Southwick and E. C. Edwards.

B. O. Dodge,
Secretary

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